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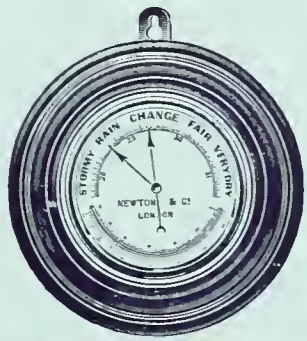

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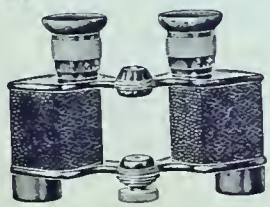

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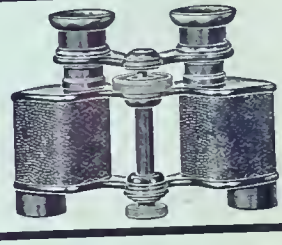

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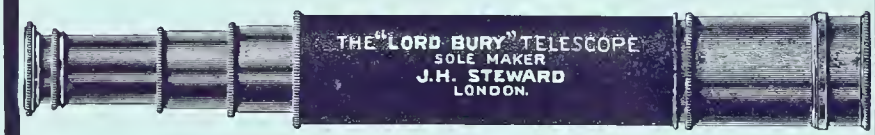

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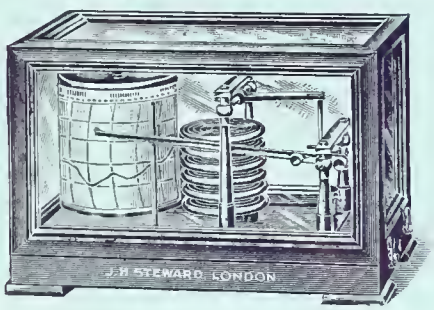

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THE MICROSCOPE AND THE PODURA SCALE.

By T. F. SMITH.

THE interesting note by Mr. R. T. Lewis, on the Podurae, must have proved pleasantly reminiscent to all lovers of the Microscope whose lifetimes, as

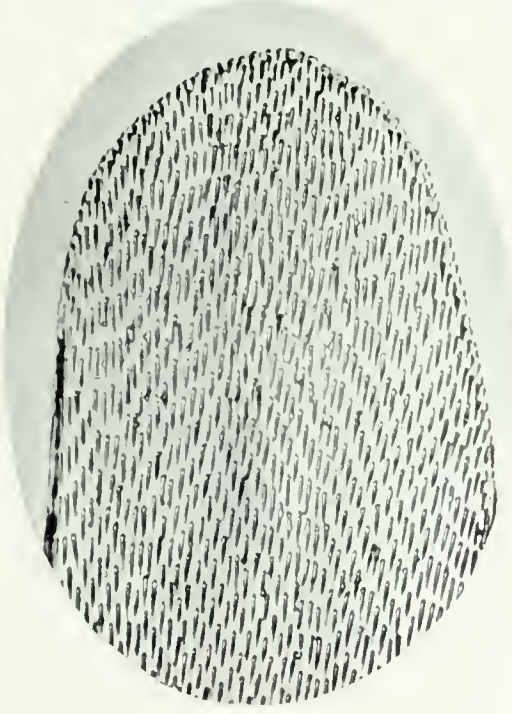


FIGURE 1.

A Podura Scale, presenting the conventional appearance of "exclamation marks" with the light-streak upon each. Taken with a dry objective, magnified 1150 times.

adults, have extended over the same period, when the battle on the structure of the scale was raging. Not that it professes to be settled even now. Mr. Lewis speaks of the scale as being a common test before the introduction of the high power oil immersion and apochromatic lenses, and rightly so, yet, since 1887 there have been, at least a dozen independent communications to the Royal Microscopical Society, on the subject of this scale. They have this peculiarity in common, too, that each observer has a theory of his own to promulgate. The last was by that eminent microscopist, Mr. E. M. Nelson, so recently as 1907.

In spite of these labours, it cannot be said that the employment of wide apertured objectives upon the scale have been justified. Indeed, with two or three exceptions, it cannot be said that a wide aperture has been employed at all. The pages of *The Journal of the Royal Microscopical Society* will bear testimony to this, where, photographically, all that has been attempted is to produce the conventional "exclamation marks." Now these are

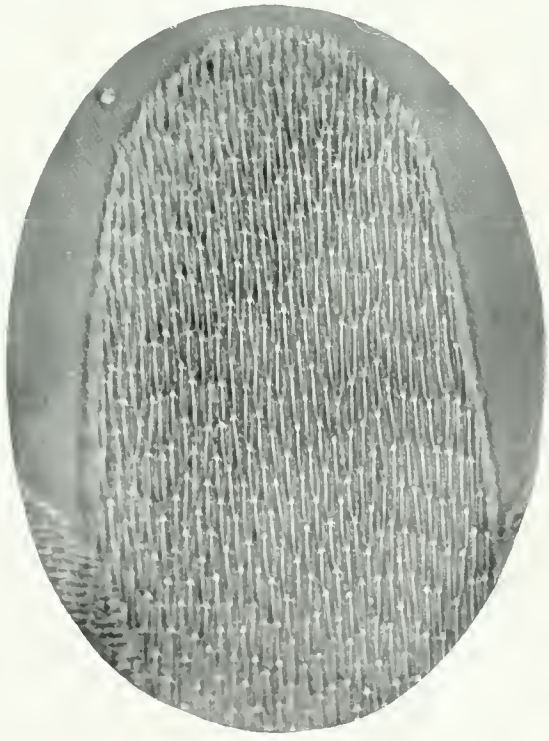


FIGURE 2.

A small Podura Scale, showing secondary structure. Taken with a Zeiss 2 m.m. objective of 1.40 N.A., magnified 1750 times.

essentially, when an oil immersion is used, either the production of the objective thrown very much out of adjustment, and much stopping

down; or, the object being on the slip instead of in optical contact with the cover glass, when the objective acts, as Mr. Nelson well remarked in one of the discussions, rather worse than a dry one. That the best results cannot be obtained under such circumstances, the writer trusts the photo-micrographs reproduced here to illustrate this article, will prove.

papers have been read, more discussions raised upon its structure, than on any other six microscopical subjects together. After occupying this position of honour so long, a pity indeed would it be were it now deposed.

Mr. E. M. Nelson, in his very interesting paper, published, with a plate of illustrations, in *The Journal of the Royal Microscopical Society* for 1907,

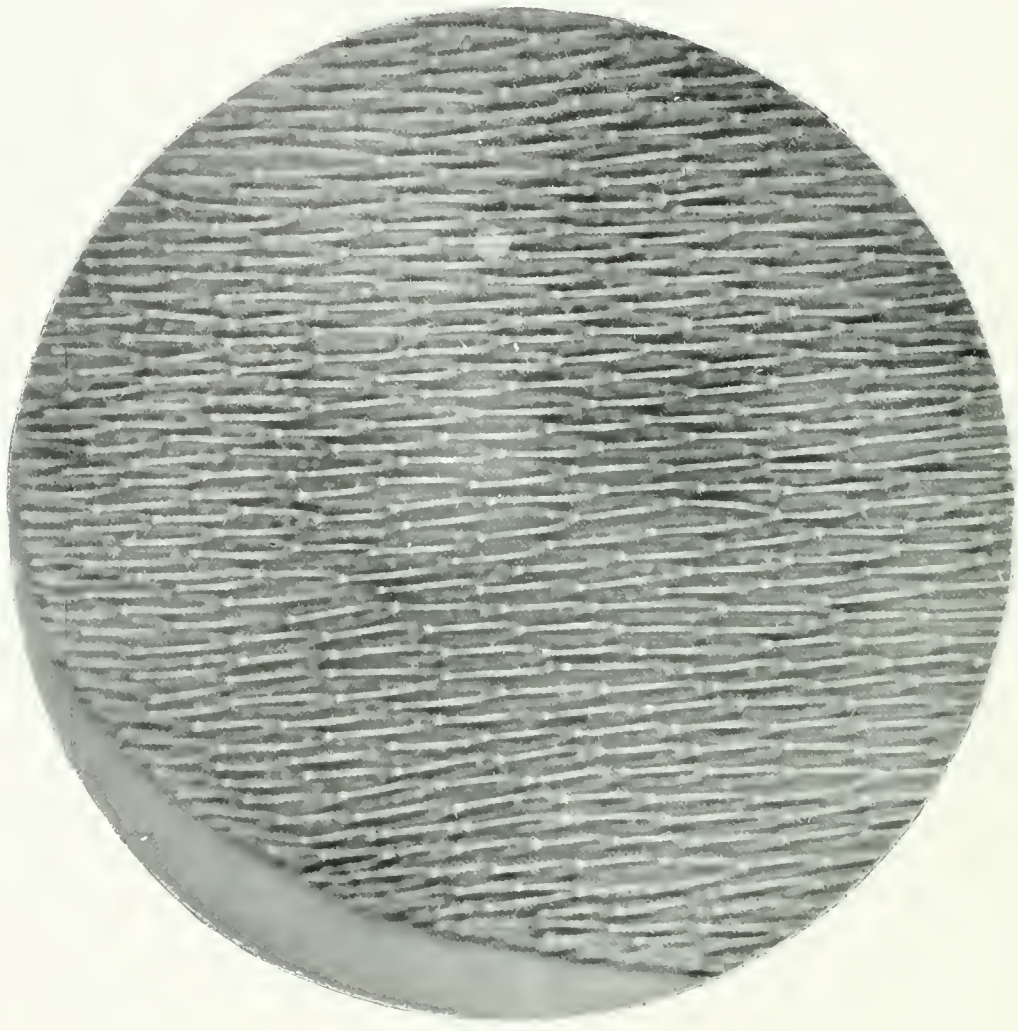


FIGURE 3.

Part of a Podura Scale, first taken at 1750 diameters with a 2 m.m. apochromatic of Zeiss of 1.40 N.A., and further enlarged to 3500 diameters.

The interest in the structure of the Podura scale, as a test object, dates back almost to the time when the microscope objective first became achromatised; that is, effectively so. The attempts at achromatising began from 1774, but the value of aperture not being understood then, nor indeed until Dr. Goring first called attention to it in about 1827, they were all more or less abortive. The first achromatic objective of English construction was made by Tully, of Islington; some three years after this scale was being used as a test object. As to the scale itself it may safely be said that more summarises the theories of different observers on the structure, thus:—

1.—Watered Silk, T. Carpenter, 1827.
2.—Wedge-shaped Spines, Dr. Goring, 1830.
3.—Featherlets, John Quekett, 1848.
4.—Indian Clubs, R. Beck, 1865.
5.—Beads, Dr. Pigott, 1869.
6.—Knobbed Heads, F. H. Wenham, 1870.
7.—Pins, T. F. Smith, 1888.
8.—Cuneiform, E. M. Nelson, 1907.

To which may be added,

9.—Perforations, Alfred Letherby, 1895.

Here is variety enough, in all conscience, to bear testimony to the puzzle of this scale. We find the strange anomaly in its condition, that while contributing so much as a test to the elucidation of structure in almost all other objects, it yet persistently keeps the secret of its own intact. A bold man indeed would he be, who says that he knows the scale in every point.

In the earliest days, illustrated by the first two examples in Mr. Nelson's list, nothing seems to have been made out beyond longitudinal and oblique lines, and these only to be seen by oblique light. Andrew Pritchard, writing in *The Microscopic Cabinet*, dated 1832, says: "I have never been able to see the lines on them with a power much below three hundred and fifty (that is, one thirty-fifth of an inch focus), and therefore microscopes of a lower power cannot be expected to show them, except of a very superior quality: for it must constantly be kept in mind that the instrument is best which exhibits an object with the *least amplification*, all other things being equal. . . . But they are most easily made out, by the simple light of a candle, in the aplanatic engiscope (microscope) if it possesses an angle of aperture of about fifty degrees (about 43. N.A.) exhibiting all their delicate minutiae with precision."

FIGURE 4.

A Ruptured Podura Scale, first photographed at 1750 diameters, and further enlarged to 3500.

Nothing can prove of greater interest to the student of historical microscopy than comparing illustrations in the infancy of the science with what we know now. That the lines on the Podura scale could only be seen under a power of three hundred and fifty diameters is a statement astonishing until we remember that it was only with an objective of one-fifth inch focus that they were able to obtain a sufficient aperture to resolve them. The same can now be seen with an inch and a power of one hundred. By the time of Quekett's first edition of his *Treatise on the Microscope*, dated 1848, the appearances seem to have developed into the now familiar "exclamation marks." This, as far as the lower magnifications are concerned. It is difficult to explain that figured under a power of one thousand two hundred and fifty diameters, called by Mr. Nelson "featherlets," except by supposing that in Quekett's time deep eye-piecing was not to be depended upon.

FIGURE 5.

A Podura Scale, isolated "pins," magnified 1750 times.

So it remained until 1869, when Dr. Royston Pigott startled the microscopical world by his celebrated theory of the beaded structure of the scale. Here the "exclamation marks" had disappeared altogether, and in their places were rouleaus of beads crossing each other at oblique angles, on different levels; the upper ones dark and the lower lighter in colour. That is, according to Dr. Pigott, who in illuminating his object violated every principle we now think necessary to produce a truthful image. According to Mr. Browning, who had seen the method, no condenser was used; the light was from a common paraffin lamp; the object was seen under a very deep eyepiece, and a draw-tube extended to sixteen inches. The most astonishing thing is that any microscopist in his senses should have accepted the theory. Many at that time, however, had "beads" on the brain, and would accept any theory of structure with beads in it, for any object. For some five or six years the subject was debated, for and against, as can be seen in *The Monthly Microscopical Journal*. Even Dr. Woodward threw his great weight into the conflict, though more as an arbitrator than taking sides: photographing the same scale, first with central illumination to show the "exclamation marks," with no trace of beading; and then, with oblique light and the focus slightly lifted, to produce beads. It was open then to anyone to take his choice.

Quoting now from Mr. Nelson's paper: "In 1888 T. F. Smith proposed a new image with a large axial cone and white dot focus; he says that the exclamation marks are precisely like pins, and that with this illumination the surface of the scale between the exclamation marks exhibits structure; oblique lines can be seen springing from the head of a pin and running to the points of the pins above them on either side."

This fairly represents the appearance of the scale as the writer knows it now, as photographed and shown in the enlargement here, from the original negative. On the other hand it is more than he had discovered at the time of reading his first paper

before the Quekett Club in 1888, and reproduced in page 499 of *The Journal of the Royal Microscopical Society* for the same year. It is important to remember this, because the new structure, visually, is entirely the result of aperture, and not until some four years later, not indeed until after acquiring a 2 mm. apo. of Zeiss, of 1·40 N.A. was the secondary structure seen and photographed. The "pins" were first discovered by a cheap one-twelfth inch oil immersion by Swift and Son, of 1·25 N.A.; the interspaces blank. The apochromatic filled the interspaces with structure, proving the scale to be as worthy a test as ever. In all cheap oil immersions now the N.A. has been increased to 1·30, and what with that and the new Jena glass used in their construction, the secondary structure should be easily seen. Two conditions, however, are necessary to see the scale under the most favourable aspect. It must be in optical contact with the cover, and the lens adjusted to the proper tube length. With the scale sunk to the slip there is an interval of air between, causing the oil immersion to work no better than a dry glass. For comparison between the photo-micrographs and enlargements, illustrating the present article and small cone work, the reader is referred to three reproductions of photo-micrographs and an enlargement of the scale, facing page 305 of *The Journal of the Royal Microscopical Society* for 1892. It is for him then to make his own comments.

When one proclaims a new doctrine one should justify the faith within him, but if the writer is asked to explain the nature of the secondary structure, he can only say he does not know. Yet he is prepared to pin his faith upon "pins," and for the reason that one of the prints, in fact two, show positive evidence no diffraction effects can explain away. It might be possible, for instance, to deny the existence of a bone in a human leg, but if that bone was broken and protruding through the flesh, denial would be no longer possible. To compare small things with great, this is just what has happened to one of the scales. Ruptured by bending on itself, two of the "pins" are left sticking out by themselves from the general line (see Figure 4). Another print exhibits isolated "pins," photographed from a shewn slide of scales (see Figure 5). Unfortunately both slide and negative have vanished into the limbo of lost things, and, consequently, an enlargement cannot be produced. Of course, an enlargement can show no more detail than the original negative; on the other hand it can make that detail much more apparent and striking.

It is only paying a proper tribute to one pre-eminent in both branches of photography, to say that Mr. Alfred Pringle was the first to photograph the "pins," only he called them "French nails." Unfortunately the print he sent the writer is also gone to the limbo of lost things, neither can Mr. Pringle find the negative, nor a print, though remembering the conversation passing between us perfectly. Coming fresh to the microscope, having no conventions to hamper him, he simply photographed what he saw under an oil immersion of 1·25 N.A., only to find the result ridiculed. His photograph did not, however, show secondary structure, and in this respect the writer believes his own to be unique.

Mr. Nelson's remarks on his own interpretation of the scale are as follows: "An examination of a Podura scale in a critical manner with modern apochromatic objectives, illuminated by a full axial cone from an achromatic oil immersion condenser, shows the exclamation marks have V-shaped heads, causing them to assume the appearance of the cuneiform marks on the Assyrian inscriptions. Both the pin and knobbed heads of the exclamation marks are due to some obliquity in the illumination, especially in a direction from root to tip. When the full axial cone is perfectly central the knobbed and pin-shaped heads disappear."

In answer, the writer can only say that the supposition that Mr. Pringle and himself were employing oblique light all the time they thought they were using central, is one he cannot for himself accept on a simple *ipse dixit*.

THE BRITISH SCHOOL OF ARCHAEOLOGY IN EGYPT.

THE exploration work of Professor Flinders Petrie and his students was carried on during the past year at Memphis and Medum. At the latter place a number of sculptured tombs were found, and one mastaba had two chambers covered with incised sculpture which was inlaid with coloured paste. This method of decoration has never been met with elsewhere, and is looked upon as one of the early experiments which was never repeated. The age of the tombs is that of Sneferu, the last King of the third dynasty (4650 B.C.) The tombs were all removed: one of the special chambers remains at Cairo, while the British School of Archaeology was allowed to retain the other, and parts of the sculpture were shown at the annual exhibition of the antiquities brought home, at University College during July last. The task of the School included the removal of a hundred feet of sculptured wall, ten feet high, as well as of the massive roof. At Memphis the palace and temple of Ptah and the pottery kilns were examined. The temple is the most important, historically, in all Egypt, and Professor Petrie is to be congratulated upon overcoming the scruples of the owners, who at first were reluctant to allow excavations to be made, and in securing contracts in connection with a good deal of the ground. Blocks bearing portraits of Amenhotep III. and Amasis were respectively found in two sanctuaries of quartzite sandstone which were brought to light in the course of the excavations made in the axis of the temple.

EARTH TIDES.

By J. A. HARDCASTLE, F.R.A.S.

THOSE who take up the study of some subject after their school-days are over, usually have to dispense with a teacher, and, in the case of astronomy, such people have recourse to books of a general descriptive nature, which carry them superficially over the whole range of the subject. Many such books are unquestionably stimulating, but they cannot possibly be otherwise than shallow, and it will be well to provide an antidote by picking up one thread and following it as far as possible, going as deep as the equipment, both mental and instrumental, will permit. In this way it is possible to penetrate into the real spirit of science, or rather to allow the spirit of research to penetrate into one's soul. For instance, the student will thus realise the sort of difficulties that have to be overcome while, by the mere perusal of the ordinary text-book, there is left on the mind the impression that there are no difficulties, because of course, everything is purposely made easy and clear. The tides are an excellent example of a thread which will repay time and trouble. Any almanac will give the times of high water, and in those columns will be found faithfully reflected every vagary of the Moon's movements. The disentangling of them requires no abstruse mathematics: interest and perseverance are all that is needed.

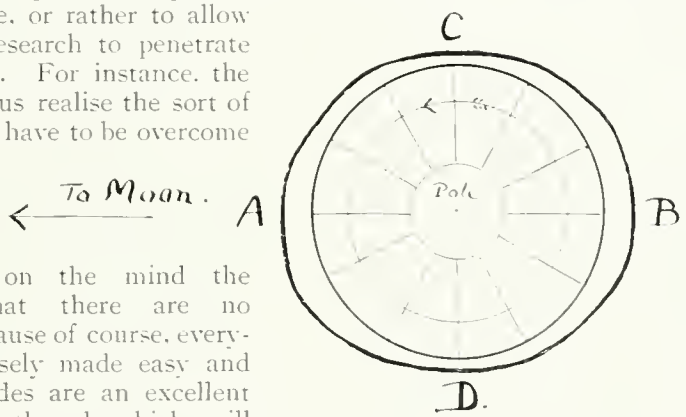


FIGURE 1.

In the history, too, of astronomy, the tides have played an important part. Newton's explanation of them forms one of the most brilliant sections of his *Principia*. As clearly as the mythical apple they demonstrate that the forces that we know on earth are not limited to the earth: it may be confidently asserted that those forces bear sway throughout the solar system, and, as we may now add, in the systems of the double stars. Perhaps some day we may show that gravitation works even across stellar distances, but it is well to remember that at present we have no evidence of this. We can, however, endorse the poet's generalisation :—

> That every law that moulds a tear
> And bids it trickle from its source,
> That law has formed the earth a sphere,
> And guides the planet in its course.

In the history of the tides the most fascinating chapter will undoubtedly be that which tells of the researches of Sir George Darwin. He has shewn us that every high water that washes our coasts is a manuscript note from the hand of the Moon herself to tell us of her life-history—an autobiographical note—if we would but read, if we could but understand. We may give a literal meaning now to Addison's words :—

> Soon as the evening shades prevail
> The moon takes up the wondrous tale,
> And nightly to the listening earth
> Repeats the story of her birth.

And we must now approach the latest chapter in the history of the tides. It announces the regular upheaval of the solid crust of the earth twice every day through a range of perhaps six inches.

That something of the kind must be taking place has long been suspected from general inferences about the forces exerted by the Sun and Moon, and the known properties of the materials of our Earth, and it has been cynically pointed out that no moment is fraught with greater danger to science than when some theoretical prediction is awaiting the confirmation of actual observation. No one, however, who reads the paper by Dr. Hecker in the *Publications of the Prussian Geodetical Institute* will suspect him of self-deception in thinking that he has obtained direct evidence of the existence of tides in the solid crust of the Earth.

In order to appreciate the magnitude of the triumph it will be necessary to realise the nature of the forces to be investigated and some of the difficulties to be contended with. And this is, after all, true in every piece of scientific research: the real interest lies not in the results but in the methods. It is perhaps one of the greatest disservices that writers who popularise such subjects habitually render, both to the public and to the cause of science, that they represent the final outcome of some investigation as the one and only point which matters. It is probably, on the contrary, the one thing which the future will repudiate.

What is a tide? The answer that first suggests itself is the regular ebb and flow of the water over the sands or beach. This is what strikes the eye, and it requires but little thought to see that this flow of the water is due to the rise and fall of the whole sea level in that neighbourhood. The sea, in fact, becomes deeper and shallower twice each day. The usual diagram to represent the tides is shown in Figure 1.

For simplicity the action of the Sun is here ignored: the greater depth of the water at A and B represents the two tides that the Moon causes. It is of course obvious that the diagram assumes that the whole Earth is covered with water, and also

exaggerates the depth of the water and the rise of the tide; one foot increase at A and B and six inch decrease in depth round "the waist" CD., is about the observed range of the tide. We are looking at the Pole and so the outer circle is the Equator; an inhabitant living on the Equator will be carried round as the arrow shews and find himself in high water followed by low water twice each day. Thus the diagram fits well with the experiences of the inhabitant on earth. Does it, however represent (exaggerations apart) what we should see if we were away outside the earth, as supposed by the diagram? Speaking very roughly it does. Strange as it may seem, there are always two tides, one on the side

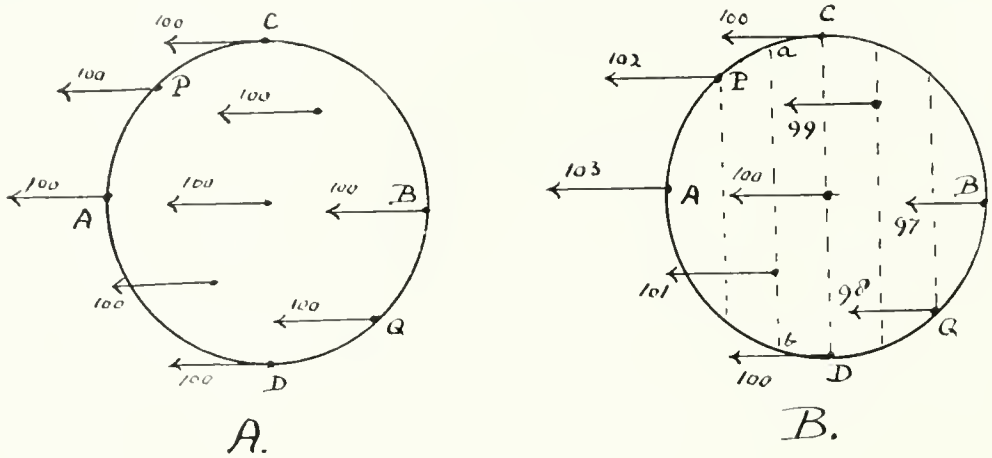


FIGURE 2.

near the Moon and one on the far side. It may be frankly admitted that the tide on the other side is a very puzzling thing, but for the moment we may content ourselves with stating that it is there, and leaving the explanation alone.

Once again, then, what is a tide? Speaking from the wider point of view of a celestial giant surveying the Earth from some point of vantage in space, we will now say that it is a deformation of the surface of the ocean. We are not concerned whether the water flow or not, but only with the change of shape of the surface. The figure, as it is called, of the Earth ceases to be round, or spherical, and becomes egg-shaped. We will think of an indiarubber ball crushed inwards round the circle CD (Figure 1), and drawn outwards at A and B. The forces that squeeze and draw the Earth are called tide-generating forces, and the question at once arises: To what are they due?

Newton taught us that the tides are due to the attraction of the Moon, but at least two thousand years ago they that went down to the sea in ships on the coasts of the Atlantic knew that the tides were connected with the Moon. What Newton did was to *include* the tides in his one comprehensive and coherent theory of gravitation. He and his successors, however, thought only of the water as being influenced; they considered the land as rigid. We know how a cane will bend, and we call it elastic; we know that an iron bar will not bend, and we call it rigid; but it is all really a question of more or less: even the bedrock that we use as a proverb for steadfastness does yield. Put it under sufficient stress and it will be strained. Stress and strain are technical words that are, unfortunately, used loosely in common conversation. A stress is a force, a strain is a resulting deformation, and we may conveniently divide what follows into the two chapters: The stresses at work, and the strains that they produce.

We shall have in mind in our discussion a very soft indiarubber ball or limp air-balloon, and we shall clear the ground by making two preliminary statements.

(1) It will only be strained when different parts of it come under different degrees of stress. If you have to lift a very limp or flimsy thing you certainly must not hold it by *one* point if its shape is to be maintained; you will support it at as large a number of points as possible. That is how the water supports a ship; if the ship strikes a rock and rests on it and the tide falls, the ship's back will probably be broken by the stress of its own weight. To avoid *all* strain you should, theoretically, support every point with equal force. Our first statement, then, is that an object is under no strain if all its particles are acted on by equal forces.

(2) In such a case it matters not in the least how great the forces are. If they are great the object *as a whole* will be urged on more rapidly, but this has no effect at all on the shape of the object.

When we say equal forces we include the idea of parallel forces, for it is clear that if the forces are not parallel they will exert some crushing or tearing influence, from which strain will result.

We shall apply these two propositions later on, but our investigation of tide-generating forces must begin by a comparison of the two diagrams, A and B shewn in Figure 2. A represents an Earth of which all the particles are being urged by equal forces of intensity, 100. In B the forces vary from 103 to 97, but the average is clearly still 100. This is not far from the actual truth: for the Moon pulls

the particles that are near her harder than those which are further from her, and somewhat in the proportions given in B.

Now the effect in both cases, as far as urging the Earth *as a whole* is concerned, will be precisely the same for both sets. The great difference lies in the fact that under the first set there is no strain at all, while there must be strain in the second case. It will be impossible from mere common sense and experience to say what sort of change of shape will take place in case B, but there must be some strain, and we shall presently shew what its nature is.

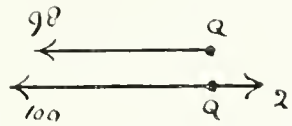


FIGURE 3.

Of the two diagrams A and B we may say briefly: A will shift the Earth as a whole without distortion, B will shift it in the same way, but with distortion. A, therefore, represents for us all that is irrelevant in B. If we can subtract all that there is in A from what there is in B, we shall have only the distortion remaining.

But how can one subtract one picture from another? It is not difficult. Let us take the particle marked P. In A it has a force of 100 on it; in B it has 102. Subtracting A from B we shall have just a force of *two* acting on P to the left. It is that *two* that makes for distortion. That is at any rate easy. But now take the particle Q. Here we have to subtract 100 from 98. We do it by recourse to a very simple expedient. In Figure 3 we see two particles. One is urged by a force 98 to the left, and the other by two forces, 100 to the left and 2 to the right. Surely both particles will behave in exactly the same way, and so in the case of Q we may replace 98 by 100 to the left and 2 to the right. Then if we subtract 100 to the left, we have left *2 to the right*. This then is the distorting force on Q.

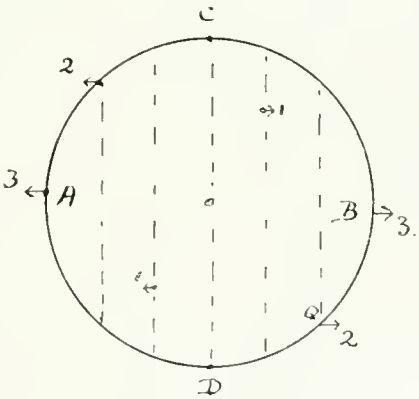


FIGURE 4.

Carrying out the work on all the other particles in the same way, we obtain the diagram of forces in Figure 4. These are the tide-generating forces produced by the Moon's attraction.

It should be noted that only nine particles are marked, and are taken as representing all the others. The intensity of the Moon's attraction marked in A (Figure 2) depends only on the distance that the particle in question is from the Moon: it does not matter whether it is on the surface of the Earth or in the interior. The force of gravity cannot be screened off, every particle of the Earth feels it, whether on the near side or far side, on the inside or the outside. It is easy to forget that every drop of water, even to the bottom of the ocean, feels the tide-generating forces, and it is still easier to forget that every particle deep in the body of the Earth feels them too. The intensity of the force in Figure 2 for every particle on the dotted line A-B will be the same; A-B represents a section through the earth, all points of which are equi-distant from the Moon.

In Figure 4 we have before us the simplest form of the tide-generating forces produced by the Moon's attraction, but the diagram is not really complete, our intention being only to give a rough idea of how the tides are produced. We can see that the general effect is to urge every particle on the near side of the Earth towards the point A, and every particle on the far side towards the point B, and thus we account for the two tides. Both form part of one single process, and a teacher should therefore be careful not to give any explanation for tide at A which will not also explain the tide at B. The student who confesses that the tide on the far side is a mystery to him, can certainly not rightly understand the tide on the near side.

It is also remarkable how small the forces are that make the tides. Only 3% is at most available for the purpose. The tides are an excellent illustration of the fact that phenomena are often the result of forces which nearly balance or which interfere with one another, and we see only the effect of the residual or outstanding difference. If we had space enough to carry out the next step we should see that even the forces on Figure 4 must be cut down considerably. It must suffice now if we point out that the Force 3 at A merely tends to lift or lighten the water there; the intensity of the force is one eight-millionth of the weight of the water, so we can only say that it tends to lift, but it clearly does not tend to shift that water, and so has no effect whatever in producing a tide. Had there been a force acting to the left on the diagram at C it would have been very efficient in shifting the water, but just there, as we see in Figure 4, the tide-raising force is zero. Accordingly the forces that are chiefly accountable for the heaping of the water at A and B are forces like that acting at P and even those are wasting a good deal of their energy in trying to lift the water. The final consequence is that the greatest tide-generating force anywhere on Earth is only one twelve-millionth of the weight of the particle which it tends to shift. It is very remarkable that a tidal wave should ever be formed at all, and not surprising that the solid crust gives no noticeable evidence of strain under these forces.

The foregoing paragraphs give so brief an account of a really difficult subject that it can only be hoped that they will leave a general idea of the process of tide-raising; but they will hardly fail to leave the impression that the forces at work could not have been guessed at by common sense method, and that it is a very incomplete, if not misleading, description to say baldly that "the tides are caused by the

attraction of the Moon." Furthermore, the forces are inconceivably small, and, finally, they actually act away from the Moon over one half of the Earth. Remembering that the centre of the circle (Figure 1) is the North Pole we can see that a drop of water carried round the world from C through A D B and back to C in twenty-four hours will for the first six hours from C to A feel itself urged eastwards: at A it feels no force except a slight lift that cannot effect any movement. From A to D it is urged westward and again is left in peace when it gets to D. From D to B the force on it is eastward, and from B to C it is westward. And these forces act not only on the water but on everything on or in the Earth.

It has long been wished to obtain direct evidence, by the help of some instrument, of these tide-generating forces: the ocean alone shews them and that is hardly a convenient instrument for research. Many attempts have been made, and in particular there was the pendulum constructed by Sir G. Darwin and his brother, at Cambridge, which was of extraordinary delicacy, and, in fact, it responded to such tiny forces that it was impossible to say what parts of its movements were to be laid to the charge of the Moon. The worst trouble was the warping of the whole solid crust every day in consequence of the heating effect of the Sun. It is easily seen that if a plummet be hung just over a mark by means of a supporting scaffolding and the scaffold be then tilted a little, the plummet will travel away from the mark. If the tilt is imperceptibly small it will look as if the plummet had been pulled away by some small attraction. This is called a spurious deflection of the pendulum, and these tiny tilts of the earth's crust are the great source of difficulty. Professor Darwin found that his own weight as he approached his instrument, made a perceptible dimple in the earth's surface causing a tilt towards himself which the pendulum duly recorded.

As already stated, Dr. Hecker has succeeded where Darwin failed. Not only has his pendulum shewn clearly that it swings twice each day under the influence of the Moon, but indirectly it also shews that the whole continent of Europe rises and falls twice a day much in the same way that a raft would rise and fall with the tide. That simile will suggest one at least of the difficulties to be contended with. How could a sailor on the high seas study the tides? How could he detect them at all? So also no one can measure *directly* the rise and fall of the earth's surface: it must be done in an indirect way.

Dr. Hecker's method was in principle the same as Sir G. Darwin's. He measured the deflections of a pendulum from a vertical; and, as we have seen, the pendulum will move for two quite distinct reasons: (*a*) Any tilt of the whole surface on which the instrument rests will produce an apparent deflection, (*b*) Any attraction or drag horizontally will produce a real deflection. Tilting will be produced not only by the warping effect of the Sun's heat, but also by any load placed in the neighbourhood of the instrument. A rise in the barometer means an extra load of air; the tilt so produced may amount to just about as much as the lunar pull for which we are looking. The addition of many thousands of tons of water on the coast at high tide will produce a tilt far inland such as the instrument will quite clearly record. We shall presently see how Dr. Hecker contended with these difficulties.

Dr. Hecker's success must be attributed to two reasons. He carried on his work for more than two years, and, secondly, he placed his instrument at a depth of 80 feet in a disused well-shaft. The great advantage of this position was that the warping due to the Sun's heat was reduced to one-seventh of what it would have been on the surface, but even so its effect far exceeded that of the direct pull of the Moon, which was the object of the search.

The instrument that was used by Dr. Hecker at the bottom of the well deserves some words of description. It is called a horizontal pendulum, because the swinging takes place horizontally like a gate. It is in fact extremely like a well-hung gate on two hinges. Such a gate will be so hung that it closes itself by its own weight, and this is attained by very slightly inclining the hinge post inwards towards the other. If, after completion the hinge-post settled a little and sloped outwards (at right angles to the closed gate), clearly the gate would have a new position of rest, and if the post were very nearly vertical the gate would be sensitive to an extremely small change of slope in the post, as long as that change took place at right angles to the gate when at rest. An additional slope towards the gate would have no effect on it. For this latter reason a pair of these pendulums must be used, their positions of rest being at right angles to each other. Such a pair would give records that practically come to the same thing as the ordinary plummet, which can be deflected in any direction, but they are infinitely more sensitive. Dr. Hecker's "gates" are nine inches long, but are as efficient as a simple vertical pendulum as high as Mont Blanc. They enabled him to read easily the thousandth part of a second of arc.

These pendulums, in their well eighty feet deep, automatically recorded their deflections during a period of twenty-nine months from December 1902 to May 1905, and at the end of that period the records were discussed.

The greatest movement of which evidence was found was quite unexpected and very annoying. The instrument had been steadily tilting over more and more in one constant direction throughout the entire period. This was found to be due to settlement of the whole well-shaft due to cracks in the masonry. Fortunately, the movement was so regular that it was possible to allow pretty accurately for it.

The next obvious feature was a daily swing, and this was attributed to the warping of the whole continent by the heat of the Sun, and then among several other minor disturbances was found a double daily oscillation due to the Moon. It will at once be asked how a lunar disturbance could be distinguished from all the rest, and the answer is quite

simple and quite convincing. The various causes can be assigned their appropriate effects because these causes are *periodic*. The Sun's heat comes round with absolute regularity every twenty-four hours—while the Moon crosses the sky every twenty-four hours fifty minutes. The Moon produces some definite effect when she is due South and she produces that same effect twelve hours twenty-five minutes later, when she is due North. Half-way between these two moments when she is due West and again when she is due East she produces exactly the opposite effect. This is only a repetition in somewhat different words of what we just now said about the experiences of a drop of water during one rotation of the earth. When the drop was at A the Moon for it was due South, when it was carried to D it would say the Moon was due West, setting and so on. Now at new Moon the Moon is South with the Sun at noon and each succeeding day she is South fifty minutes later and in a month she is again South at noon. Thus if the records at noon of thirty consecutive days be added together, we have a sample of thirty different lunar effects, one-half of which are precisely the opposite of the other half, and so *in the average of the thirty* the lunar effect vanishes and only the heat effect of the sun remains, and we get its value for noon. Precisely the same method may be used for one, two, three, and so on, o'clock. At any named hour of the day the Sun is at the same place in the sky, but the Moon may be anywhere, and thus the whole cycle of solar effects through twenty-four hours is obtained. Putting it briefly we may say that we group our records according to *the period* of the cause which we are for the moment considering, and the averages over several years will eliminate all other causes. When seeking for the Moon's effect we must group according to the lunar period of twenty-four hours fifty minutes; one group shall give us the average record at the moment when the Moon was South; another group shall be taken one hour later each day, another two hours later, and so on. Some eight-hundred days were recorded, and so each group will give an average of eight-hundred records, and obviously such varying influences as the height of the barometer will be eliminated in that average and, *a fortiori*, the solar effect, which varies according to a fixed but *different* period, will be removed. The following diagrams give summaries of the results of this grouping and averaging.

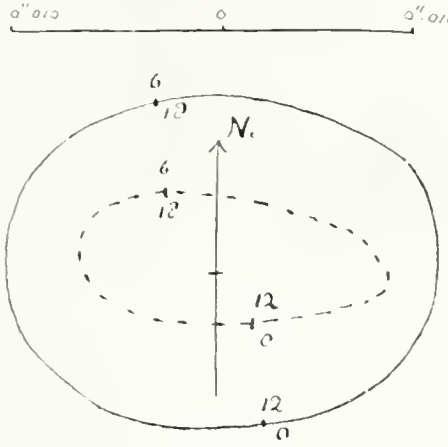


FIGURE 5.

Figure 5 gives the average doings of the pendulums when the whole series is consulted. The full line shows what would be expected of the pendulum if the lunar forces shown in Figure 4 all produced their theoretical effect: the hours at which the plummet should reach each point are given. The dotted line gives the average found, and again the hours are indicated. It will be seen that the pendulum did not swing so widely as theory expected, but otherwise it carried out most punctually what the lunar attraction commanded. It is this perfect accord between theory and observation which forms the success of Dr. Hecker's work: the pull of the moon is directly recorded by the instrument.

The perfection seems marred by the one fact that the swings are not so ample as they should be, but far from being a defect this feature is perhaps the most satisfactory point in the whole work. The theoretical curve is worked out for an absolutely rigid earth, in which no tilting, no deformation of any sort, takes place, and we have seen that a tilt will shift the pendulum in a manner quite similar to a direct pull on the plummet. If then the moon's drag produced a regular tilting taking place precisely according to the period of that drag, its presence would remain in the average: it would be a maximum when the drag was a maximum, and its effect cannot be disentangled from that of the direct drag. The deficiency in the amplitude of the swing suggests that some such regular periodic tilting is going on with an effect always contrary to that of the drag, but only great enough to diminish, not entirely to obliterate, the evidence of the drag. Can we then see any reason to expect such a deformation of the surface of the earth—a deformation which we may at once boldly call a tide since it is to be regularly periodic twice in each lunar day of twenty hours fifty minutes? If we admit any elasticity to the solid earth, the answer is clearly in the affirmative. The forces in Figure 4 are acting as much on the crust and interior as on the water and the pendulum on the exterior, and to whatever extent the crust gives, to that extent will any motion of the pendulum and any tide in the ocean be diminished.

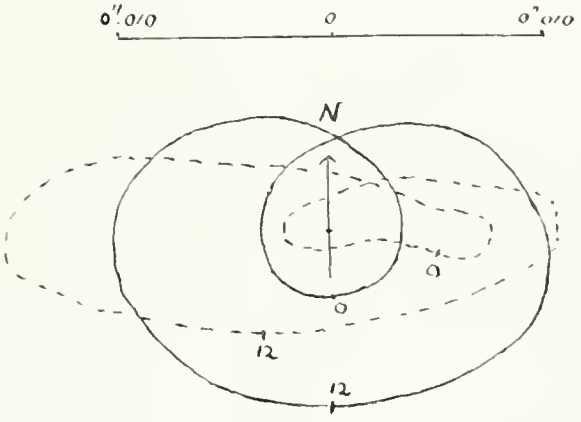


FIGURE 6.

If, instead of thinking of the behaviour of a plummet on a rigid earth we pass to the other extreme and think of the earth as a perfect fluid responding sensitively to the least stress, and if we suppose our pendulum to be on a raft floating on the surface of this fluid Earth, then it would be quite impossible to obtain any evidence of the existence of the forces shewn in Figure 4. The pendulum would never move at all: the lunar drag on it and the tilting of the raft on the deformed Earth would precisely cancel each other. This is a very difficult point, but the truth of it can be seen in a general way, if one remembers that the plumb line and the surface of the fluid are certain to be perpendicular to each other. If some force displaces the one it will displace the other in perfect harmony. Many examples of this occur in surveying work. Near any steep high hill the plumb-line is appreciably diverted, but so is the surface of the sea or of a mercury bath. The plumb-line and the surface of a perfect fluid will *always* be perpendicular to each other, and for this reason the instrument on the raft would record no movement of the plummet over the base. And for the same reason there would be no movement of the water on the coast, if the coast responded as perfectly to the solicitations of the Moon as did the water. There would then be no tides in the oceans of an Earth of which the rocks acted like a perfect fluid.

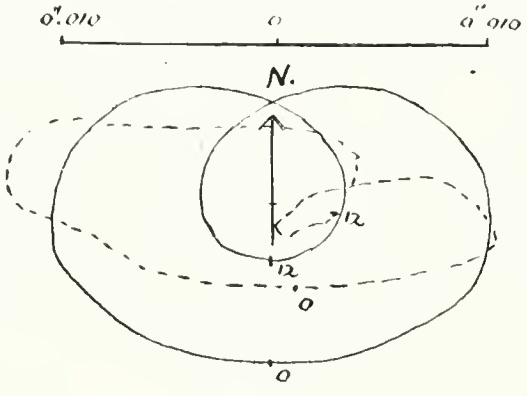


FIGURE 7.

From these general considerations, and without going into elaborate mathematics, it can be seen from the fact that the pendulum recorded less than theory predicted for a rigid Earth that some yielding was taking place in the rocks on which the instrument was based. The amplitude of the recorded oscillations was about two-thirds of that expected for a rigid Earth. This gives all that is needed to calculate the elasticity of the crust, and the result is that it must be about as elastic as steel. A bedrock of several thousand miles of solid steel will be regarded by engineers as a good enough base for any structure that they are called upon to build; so we need not be nervous when we hear that we are being raised and lowered by the elastic yielding of such foundations. The elongation shewn in Figure 1 would amount for a fluid Earth to something like a yard; for a steel Earth it would be about a foot. So if we assume that the water of the ocean acts as a perfect fluid and the crust acts as steel would, then we can see that the oceanic tides will rise and fall about two feet on the coast instead of three feet; and it is now generally held that the tides are diminished by about a third in consequence of the yielding of the crust.

A better conception of the completeness of Dr. Hecker's work will be gathered from an examination of the next two diagrams. Just as the Sun visits the Northern and Southern Hemispheres alternately, producing winter and summer, so also each month the Moon visits the northern and southern sides of the Earth. Dr. Hecker has taken out the averages separately for these "Lunar Summers" and "Lunar Winters." Figure 6 gives the doings of the pendulums when the Moon was far North, the full line representing what is to be expected on a steel Earth; and similarly Figure 7 gives the same facts for the days when the Moon is in Declination far South. In both figures both the accordances and the discordances between theory and observation are remarkable. The double loop is in good agreement: the hours against each point show perfect harmony between theory and observation: especially when it is noted that twelve o'clock comes on the larger sweep in one case, and on the smaller in the other. There can be assuredly no question that the pendulums are recording the phenomenon which was sought for. The chief discrepancy is in the fact that observed curves are distinctly dwarfed in the direction North and South. This has received close attention at Dr. Hecker's hands, and all he can say is that we must assume that the continent of Europe is less rigid in the Northerly and Southerly direction than in the Easterly and Westerly direction.

In conclusion, there will probably arise in the mind the question of Earthquakes. Will this deformation of the Earth's surface tend to produce or perhaps even to account for Earthquakes? At present it is too early to venture on any answer to the question. We must first make up our minds whether Earthquakes are rendered more frequent by greater or by less rigidity. At first sight one would say that the more rigid the structure is the less it will quake; but then, again, the more nearly it conforms to the shape that the forces on it make for, the less reason is there for it to quake, the less stress is it under. It is impossible to say from general considerations which of these facts will prevail. Earthquakes are now being largely attributed to the readjustment of the shape of the Earth in response to changed circumstances of rotation. The Earth's figure is held to be somewhat out of date; she is too flat at the Poles and possibly not quite so perfectly circular at the Equator as she should be. But in such arguments the changes of circumstances are extended over millions of years, while these Tides in the crust are recurring twice each day. For the present we must be content to leave their possible connection with Earthquakes on the knees of the gods.

PHOTOGRAPHING THE FLIGHT OF INSECTS.

By DR. ALFRED GRADENWITZ.

APART from its spectacular interest and educational value, the cinematograph is a most useful apparatus for scientific investigation, allowing such phenomena as are too rapid or too slow for direct visual inspection to be decomposed into their various phases.

A French scientific man, Lucien Bull, of Paris has developed a most ingenious method which greatly increases the scope of the cinematograph by augmenting its speed—that is, the number of pictures taken within a second—to almost incredible figures.

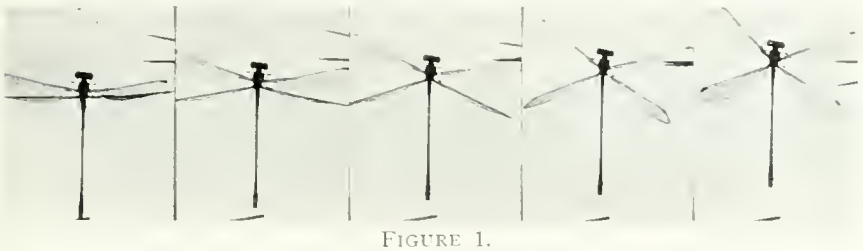


FIGURE 1.

Whereas the ordinary apparatus takes from thirty to fifty instantaneous pictures during a second, this high-speed cinematograph, in fact, enables as many as two thousand views to be taken during this short interval. It is thus possible to decompose the most fugitive phenomena, such as the motion of insect wings, as far as can be desired, and by projecting the component pictures on a screen in more or less rapid succession, to study their very mechanism. It may be said that by investigating the flight of insects, it is hoped to arrive at a true understanding of many problems met with in aviation.

FIGURE 2.

It will be readily understood that no mechanical apparatus is able to open and close the shutter of a photographic camera two thousand times a second. Moreover, the ordinary illuminants are far from possessing the intensity required for producing an efficient impression on the photographic plate or film during so infinitely short an interval.

The inventor therefore avails himself of the *electric spark*, which unites two extremely valuable properties, viz., an instantaneous duration, and enormous photographic efficiency.

His apparatus (Figure 6) consists mainly of a pasteboard roll bearing on its circumference a photographic film, and which, under the action of an electric motor, rotates extremely rapidly.

A rotary interrupter mounted on the axis of this roll breaks the primary circuit of an induction coil a certain number of times during each rotation, and each break produces an induction spark behind an optical system used for concentrating the beam of light on the objective.

After starting the apparatus, the sparks are seen to pass the more rapidly as the speed of rotation of the roller is higher. Each spark will produce a photographic picture on the portion of the film which happens to be in front of the objective. In order therefore to obtain on the film a series of images of an object situated in front of the optical system, the objective should be kept open during the time of one rotation of the roll.

The roll used by Lucien Bull is about one foot in diameter, and accordingly can receive a film about three feet in length, containing fifty-four photographs of the same size as ordinary cinematograph pictures.

In order to allow the apparatus to be operated in full daylight the rotating roll is enclosed in an octagonal box, the upper half of which can be removed for the sake of inserting the film. The objective is contained in a small wooden compartment or pointer at the front of the box, and which contains a mirror reflecting the image of the object on to the ground glass window, thus allowing the apparatus to be accurately adjusted. Before beginning the experiment, this mirror is fitted so as to leave free the way to the objective, and to provide a light-tight closure for the sighting window.

FIGURE 3.

The proper speed of rotation is best ascertained by gauging the sound given out by the sparks, which is

of remarkable purity, being comparable to that of a violin.

The optical system concentrating the beam of

FIGURE 5.

A Dragon Fly as seen coming towards the observer.

light on to the objective consists of quartz lenses which absorb far less of the chemically active rays than glass. The objective consists of a system of quartz and calcite lenses, which latter possess the same properties in regard to absorption (see Figure 7).

As the pictures obtained with the high speed cinematograph on a bright background are necessarily silhouettes, it is often difficult properly to appreciate their perspective value. Figure 4 thus shows a dragon fly in full flight which, according as the observer sees the head of the insect turned towards him, or from the opposite side, can be interpreted in two different ways. In order to do away with this uncertainty the experimenter uses a double or stereoscopic objective producing two views simultaneously, as seen with the left and right eye respectively, and which, taken together, give a perfectly plastic impression. The spark gap has obviously to be likewise duplicated so as to provide for two parallel sources of light.

As the film during an experiment should be exposed to the light of the sparks only during a single rotation of the roll, the apparatus is provided with an electrically-operated shutter, which is opened rapidly at the proper moment, and closed automatically again after the roll has performed a complete revolution. This shutter consists of a brass plate with two rectangular windows of the same dimensions and distances as cinematograph pictures. These two windows are closed previous to the experiment by a small shutter of very thin sheet steel, which is disengaged at the proper moment by a special electro-magnet so as to open the two windows. At the end of the experiment another shutter (also operated by an electro-magnet) automatically closes the windows.

The time is registered by means of an electro-magnetic recorder controlled by a tuning fork of fifty vibrations per second, the ends of which are photographed along with the pictures. However, in most cases, it will be sufficient to compare the sound given out by the spark with that of a given tuning fork in order to ascertain the speed of working.

The apparatus above described was mainly constructed for the purpose of photographing the flight of insects. All the endeavours so far made to fix this fugitive motion on the photographic plate have given only partly satisfactory results, no experimenter being able to record a successive series of instantaneous pictures. Moreover, the insect was mostly kept captive, and therefore could not be investigated under normal conditions. M. Lucien Bull has therefore made a special point of keeping the insect under test at full liberty, confining its captivity to a few seconds previous to the exposure. Again, in order to impart to its flight the proper direction towards the photographic objective, he avails himself of the fact that insects will nearly invariably fly towards the light, and therefore arranges his apparatus close to the window. The insect liberated on the opposite side of the table will fly towards the window, and on passing in front of the optical system is photographed by the apparatus. The motion recorded thus is that of the first moments of flight which are the most instructive, as their speed is comparatively low.

The insect to be photographed is kept imprisoned by a small electro-magnetic grip inserted in the circuit of the electric shutter. (See Figure 5). As soon as this circuit is closed, an electro-magnet will open the grip, thus liberating the insect, which flies away immediately.

In the case of those insects which only fly away

FIGURE 5.

A Fly held by the apparatus previous to being liberated.

after some time the experimenter uses a glass tube with slanting ends (see Figure 8), closed by an extremely light mica window which can be opened

by the insect itself. As soon as this has been done, the operator actuates the sighting mirror above

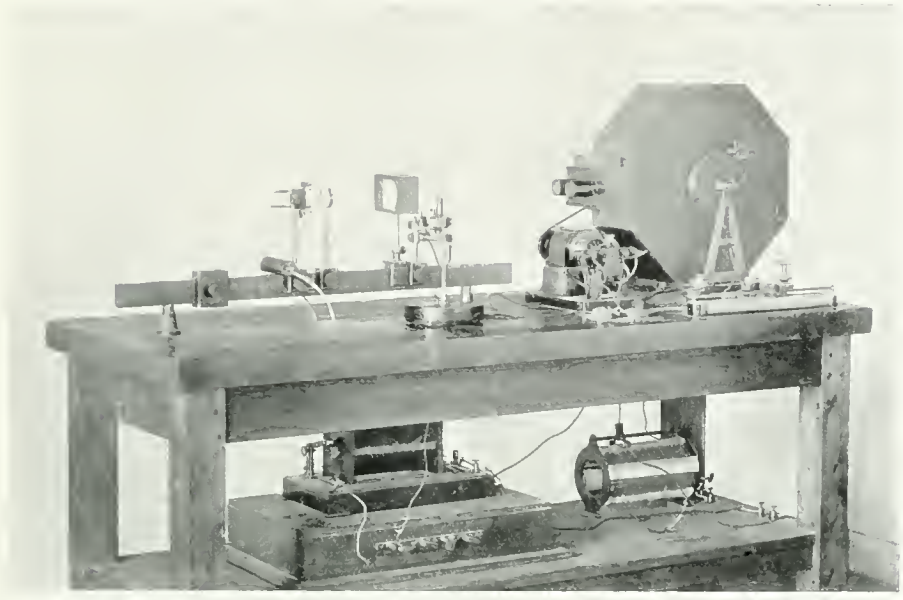


FIGURE 6. M. Lucien Bull's complete apparatus for photographing the flight of insects.

referred to, and thus closes the electric contacts which are necessary for working the shutter described.

In connection with the experiments so far made, M. Bull has found the flight of all insects to be practically alike in so far as the motion of wings is concerned. The best experiments were made on the dragon-fly which, in addition to being very enduring, possesses especially long wings which are well seen on the photographic records (see Figures 1–4). Their period of vibration also is relatively long (one thirty-fifth of a second), which allows of a considerable number of pictures being obtained during one full motion.

The dragon-fly when at rest keeps its wings in a vertical plane above its back with their axes in the extension of the transverse axis of the thorax. At the moment of flying up the insect first lowers its front wings, and afterwards its hind wings, bringing them at the same time in front of its head. The ends of the wings describe a curve turned upwards and forwards, the general direction of which cuts the flying axis at an angle of about forty-five degrees. When arriving at the end of their course the wings, which describe a more or less wide loop, will invert the direction of their motion, returning to their starting point. The hind wings describe a curve similar to that of the front ones, but for their motion lagging from one-eighth to one-quarter behind that of the former.

The back curve of the wing, so far from coinciding with the one described in the first part of the motion, cuts it towards the centre so as to form an eight. The same peculiar feature is observed in connection with all other insects tested, except that the width of the loop is variable.

The speed of the end of the wings during the middle part of each reciprocating motion is about two and a half metres per second in the case of the dragon-fly. The forward and backward motions, in opposition to the flight of birds, occur at about the same speed.

FIGURE 7. Lenses of quartz, glass and calcite showing their comparative transparency.

The variable inclination of the wings is quite interesting to observe. Throughout nearly the whole time the wing is lowered its plane is practically horizontal; when arriving at the lower end of the curve the wing undergoes a sudden rotation through ninety degrees round the front edge, returning to its starting point

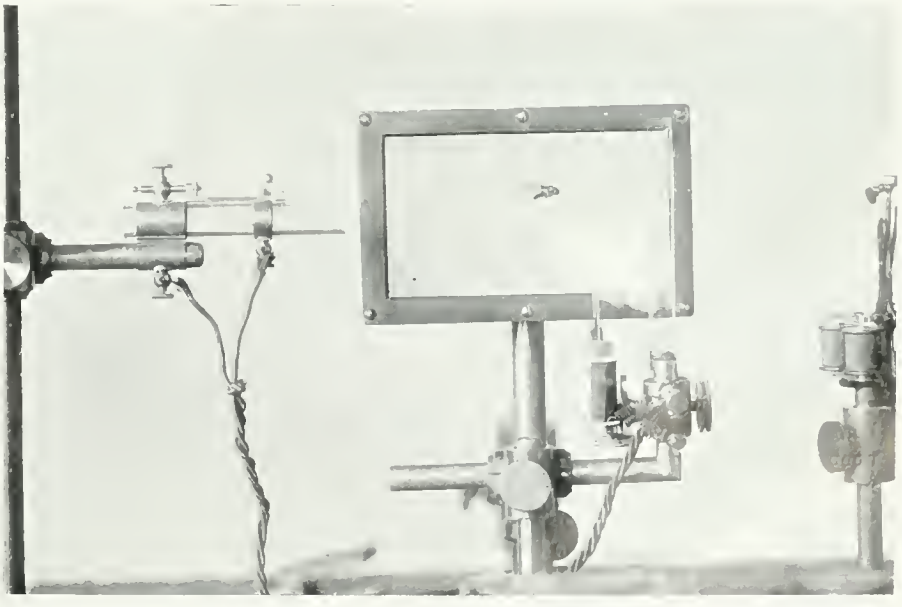


FIGURE 8. The details of the screen, and tube for retaining the insects temporarily.

in a vertical position. The lowering of the wings from behind towards the front is mainly intended for lifting the insect, while the rear motion, from the front to the back, provides for its horizontal advance.

The duration of each stroke of the wings under normal conditions is remarkably constant. Like any other muscular contraction it, however, augments with fatigue, as well as with any decrease in temperature.

The progress of these interesting experiments will doubtless bring out many other points of value for the science of aviation.

THE MARKINGS ON MARS.

By W. F. DENNING, F.R.A.S.

THE precise character of the features on Mars has given rise to ample discussion in recent years. Ever since the late eminent Italian astronomer, Schiaparelli, distinguished markings in 1877, which he termed "canals" or "channels," observers have been at variance on the matter, and angry controversies have not resulted in satisfactory issues.

Some telescopic students of this planet could see no canals at all, others could discern a few of them, while others again detected a great number, and delineated the surface with a geometrical network of lines.

The canals were also said to exist in pairs, that is, they consisted of double lines like a railway. This singular configuration was supported by several observers, and as ardently objected to by others. For more than a generation there has been a wide diversity of opinion and a war of words on the subject.

The question seemed to hardly admit of practical settlement, but the last few oppositions of Mars, and especially that which occurred in the summer of 1909, have furnished important evidence calculated to prove what the surface markings of Mars are really like.

The double canals may be said to have been eliminated some years ago, as an optical illusion. And now the network of narrow lines drawn by Lowell, Brenner and others, must similarly disappear, as incorrectly representing the visible condition of the planet. That there are dusky canal or channel-shaped markings on Mars must be admitted. Dawes and other observers noticed and figured them many years ago, but they do not exist in the same character or abundance as certain modern observers have affirmed.

M. Eugene Antoniadi, whose observational and artistic abilities are of a very high order, has laboured most meritoriously in recent years to ascertain the truth concerning the lineaments of Mars and to picture and describe the surface of the planet just as it appears in nature. With the great 32·7-inch refractor at the Meudon Observatory, Paris, he made a series of observations of Mars in 1909, and his results conclusively disprove the "systems of hard geometrically-patterned lines with which some observers have so elaborately endowed the planet." The minor details or "canals" appeared irregular, knotted and disconnected, and presented a perfectly natural appearance.

Instead of being artificial constructions, such as certain imaginative people would have us believe, we may depend upon it that the markings are the outcome of natural phenomena, and that the chimerical theories advanced to explain them deserve no credence whatever.

M. Antoniadi concluded from the experience he gained with the large Meudon refractor that the performance of a really good large telescope must invariably beat that of a smaller one. When the atmosphere was calm and definition good he found that the big instrument really possessed a great superiority over those of small size.

With the recent developments in regard to our knowledge of Mars, I think that all unprejudiced observers will cordially agree. I have viewed Mars under a similar aspect to that described by M. Antoniadi, but of course my smaller instruments (ten and twelve-and-a-half inch reflectors) afforded me a less impressive grasp of detail. Twenty years ago I ventured to summarise the results of observations at Bristol as follows: "The detail is visible in the form of irregular streaks, condensations and veins of shading very faint and delicate in some parts. The veins apparently connect many of the larger spots, and here and there show condensations which have sometimes been drawn as isolated spots. A night of good definition, however, reveals the ligaments of shade connecting them." (*Telescopic Work for Starlight Evenings*, p. 160.)

Mars is one of our nearest planetary neighbours and certainly one of the most interesting, and the recent work of Antoniadi, Molesworth, Phillips and others, in providing us with thorough and reliable charts of the planet's topography, deserves high commendation. The markings of Mars have a special significance as being parts of the planet's actual surface, while the features seen on Jupiter and Saturn are merely atmospheric phenomena. For this reason, apart from their interesting, abundant and varied character, the Martian spots and streaks merit careful study as durable formations on the globe of perhaps the most interesting planet of our system.

In reporting my observations of Mars in 1903 to the *Ast. Nach*, 3926, I remarked that "a large number of irregular dusky streaks, different in tone and direction, were observed. Some of these were very distinct, as, for example, Nilosyrtis, Protonilus, Indus, Ganges, Cerberus, Casius, and so on, while others as Phison, Euphrates, Gehon, were feeble or extremely faint and delicate. Many of them were knotted or strongly condensed in places, and particularly so at those points where either a junction or intersection of two of them occurred."

In fact, the streaks or canals were nothing like the hard dark lines depicted by certain observers. They were rather streams of shading, far from straight or equable in tone, and decidedly gave the impression of being natural formations.

Some of them were as distinct as the fainter belts of Jupiter, and I think that the terms, streak and canal, are quite applicable to the appearances alluded to, though many of them may consist of irregular details blended into bands of shading. Some of the belts of Jupiter are made up of a mass of features, and are very far from being smooth belts of dusky material. Under sufficient amplification they would be seen broken up into a great variety of "spots," strung out in a longitudinal direction. This particularly refers to the great South equatorial belt of Jupiter, which frequently exhibits a procession of condensations, bright spots, wisps and other irregular details.

THE TRIPHENYLMETHYL PROBLEM.

By HERBERT H. HODGSON, M.A. (Camb.), B.Sc. (Lond.), Ph.D. (Heidelberg).

PART IV.

THE HEXAPHENYLETHANE PROBLEM.

THE question as to the constitution of triphenylmethyl is inevitably bound up with that concerning the existence of hexaphenylethane. Since the so-called hexaphenylethane of Ullman and Borsum was proved by Tschitschibabin to be none other than benzhydryltetraphenylmethane $(C_6H_5)_2\,CH.\,C_6H_4.\,C(C_6H_5)_3$, the formula of hexaphenylethane has again entered the list of unsolved problems.

Tschitschibabin and Hantzsch have expressed the opinion that in triphenylmethyl we have the long-sought-for hexaphenylethane. The latter chemist compares hexaphenylethane with hexanitroethane, which it is impossible to prepare by the action of trinitroiodomethane on silver nitroform owing to its instability, since instant decomposition ensues. Hantzsch is of opinion that in hexaphenylethane the two ethane carbon atoms must be so feebly adhered to each other that hexaphenylethane behaves as free triphenylmethyl.

In opposition to this view of Tschitschibabin and Hantzsch, Gomberg and Cone have put forward their opinion that the real hexaphenylethane has not been prepared, owing to suitable experimental methods not having been evolved to deal with such a peculiar case of instability as one would expect. This view was fortified by the fact that Gomberg and Cone had succeeded in the preparation of the unsymmetrical tetraphenylethane, and also pentaphenylethane, which bodies have a fairly stable behaviour.

Tschitschibabin showed that pentaphenylethane appeared to decompose at its melting point, and when heated with hydrochloric acid in benzol solution at 150° was transformed into tetraphenylethane, triphenylmethane and triphenylchlormethane. Cone and Robinson have also obtained triphenylchlormethane by heating pentaphenylethane with phosphorous pentachloride.

It should not appear hopeless to consider that in pentaphenylethane, taking into account its reduced stability, we have a stepping stone towards the preparation of hexaphenylethane, since the leap from pentaphenylethane, a comparatively stable body, to the extraordinarily unstable triphenylmethyl, appears somewhat great.

Experiments, starting out from ethane derivatives in order to build up hexaphenylethane, must now decide whether triphenylmethyl and hexaphenylethane are one and the same body, or whether the hexaphenylethane formula corresponds to another, as yet unknown, stable, hydrocarbon.

Gomberg tried to prepare pentaphenylethanol by acting on a tetraphenyl derivative of ethane, *viz.*: on β benzpinacoline with phenylmagnesium bromide, but without success. In like manner to the re-action by which Ullmann and Munzhaber had prepared Aminotetraphenylmethane, Schmidlin wished to condense benzpinacone and aniline to diaminohexaphenylethane, but without result.

$$\begin{array}{c} (C_6H_5)_2.\,C\,O\,H \\ | \\ (C_6H_5)_2.\,C\,O\,H \end{array} + \begin{array}{c} H.C_6\,H_4.\,NH_2 \\ \\ H.C_6\,H_4.\,NH_2 \end{array} = \begin{array}{c} (C_6H_5)_2.C.\,C_6H_4NH_3 \\ | \\ (C_6H_5)_2.C.\,C_6H_4NH_2 \end{array} + 2H_2O$$

Even so unsuccessful was the attempt to prepare pentaphenylethanol by the action of benzophenone on the normal β-magnesium compound of triphenylchlormethane.

$$(C_6H_5)_2\,CO + (C_6H_5)_3\,C.M_g\,C_l = (C_6H_5)_2.C(O\,M_g\,C_l).\,C\,(C_6H_5)_3.$$

In the above experiment a very small quantity of a body was obtained which, although not the expected pentaphenylethanol, obstinately held magnesium. This body did not appear to have been formed by the interaction of benzophenone, but by the action of unchanged triphenylchlormethane on the β-magnesium compound.

Full of promise appeared a method by which triphenylchlormethane should be allowed to reaction with its β-magnesium derivative at a very high temperature (250° C.), and under prolonged heating. Schmidlin obtained by this method a very small quantity of a body melting at 270° C., but containing magnesium as impurity. The analysis of this substance, taking into account the ash, gave as its formula that of hexaphenylethane. The magnesium, which at first was regarded as an impurity, appeared, however, to be very firmly united. After shaking for many days with concentrated hydrochloric acid the magnesium was removed, but the resulting ash-free substance had then completely lost its ability to crystallise: in fact the molecule appeared to have been disrupted. The high melting point compound, therefore, appeared to be a magnesium compound whose composition corresponded approximately to the formula:—$[(C_6H_5)_3\,C]_2\,Mg$. On removal of the magnesium, however, no triphenylmethane was obtained, but an uncrystallisable product, and from this it appears almost certain that the magnesium is not united with the triphenylmethyl residue, but has entered the benzene nucleus.

A strenuous controversy has been waged for many years between Gomberg and Schmidlin on the one hand, who have held the opinion that triphenylmethyl and hexaphenylethane were separate and distinct substances, and on the other hand Tschitschibabin, who believes the two to be identical.

The last named author has criticised Schmidlin's magnesium compounds. He states that both the α and β modifications re-act with benzoyl chloride and ethyl benzoate, forming benzpinacoline in an 80 % yield. Contrary to Schmidlin's statement, Tschitschibabin finds the α-modification on treatment with carbon dioxide in ethereal solution to give triphenylacetic acid in a 90 % yield; the action of water on the re-action product leads to the formation of triphenylmethane in good yield. The para-benzoyltriphenylmethane obtained by Schmidlin was formed most probably by the condensation of triphenylmethyl with benzaldehyde.

Schmidlin has replied to the above criticism as follows:—Triphenylmethyl and α-magnesium triphenylmethyl chloride result when magnesium in moderate excess re-acts gently with an ethereal solution of triphenylmethyl chloride in the presence of a moderate amount of iodine. With a large excess of iodine the quantity of triphenylmethyl is greatly diminished, and if the re-action proceeds vigorously, a partial conversion of the α-magnesium triphenylchlormethane into the β-isomeride occurs. Tschitschibabin employed the latter conditions in his experiments, leading him to doubt the existence of two isomeric magnesium triphenylmethyl chlorides. Schmidlin is of opinion that Tschitschibabin's solution could have contained little or none of the α-isomeride, that his explanation of the formation of para-benzoyltriphenylmethane is impossible, and that its production from triphenylmethyl is negatived by the three following experiments. An ethereal solution of triphenylmethyl, free from the magnesium compound, does not re-act with benzaldehyde. Solid triphenylmethyl in benzene solution is equally non-re-active. The re-action between magnesium and triphenylmethyl chloride was so performed that the ethereal solution contained the triphenylmethyl whilst the α-magnesium triphenylmethyl chloride was present exclusively in the precipitate; the solution did not re-act with benzaldehyde, whereas the precipitate yielded para-benzoyltriphenylmethane. The argument that the so-called α- and β-magnesium triphenylmethyl chlorides react in the same way with benzoyl chloride and ethyl benzoate is therefore countered by the statement that, under the conditions, the labile α- form changes into the stable β-isomeride. Tschitschibabin has made a further reply in which he maintains that the reaction with benzaldehyde is capable of explanation without the assumption of two magnesium isomerides. He has already suggested that this substance may re-act with benzaldehyde normally yielding β-benzpinacoline, and also abnormally as follows, whereby para-benzoyltriphenylmethane is produced.

$$C_6H_5.CHO + (C_6H_5)_3C\,Mg\,Cl \longrightarrow OH.CH.C_6H_5C_6H_4C(C_6H_5)Mg\,Cl$$

$$\longleftarrow CH.C_6H_5(OMg\,Cl).C_6H_4.C(C_6H_5)_3$$

Illustrations of such abnormal condensation are found in the re-action between formaldehyde and magnesium benzyl chloride, whereby both benzyl carbinol and o-tolyl carbinol are formed, and also in the re-action between benzaldehyde and magnesium benzyl chloride, which leads to the formation of phenylbenzyl carbinol, phenyl-ortho-tolyl carbinol and phenyl - para - tolylcarbinol. Tschitschibabin also obtained both benzpinacoline and para-benzoyl-triphenylmethane from solutions of magnesium triphenylmethyl chloride which, according to Schmidlin, should contain only the β-modification and also from solutions which should contain only the α-form. Hence he draws the conclusion that there is no evidence for the existence of the isomeric magnesium triphenylmethyl chlorides.

To complicate the problem still further, Schmidlin in 1908 made the remarkable discovery that there exists a colourless and a coloured triphenylmethyl. A freshly prepared solution of pure triphenylmethyl in benzol remains colourless for a few seconds and then gradually becomes orange-yellow. The colour of the solution disappears, however, when the latter is shaken with air, re-appearing again after a time and again disappearing when agitated with air, until finally the solution remains perfectly colourless. The conclusion is therefore drawn that the yellow solution of triphenylmethyl contains both a coloured and a colourless form of triphenylmethyl. The two forms are convertible one into the other and are present in solutions and in the solid state in a definite condition of equilibrium, which depends both on the nature of the solvent and on the temperature.

The disappearance and re-appearance of the colour of solutions of triphenylmethyl is due to the fact that the coloured variety oxidises far more rapidly than the colourless form. On shaking an ethereal solution of triphenylmethyl with air, the yellow form is oxidised, yielding the peroxide, which, being insoluble in ether, may be filtered off; the colourless filtrate quickly becomes yellow, and on shaking yields a further quantity of peroxide. It was found from the amount of peroxide formed, that an ethereal solution of triphenylmethyl contains roughly ten times as much of the colourless as of the coloured form. The latter substance must consequently have an exceedingly intense colour.

The yellow form also absorbs iodine far more rapidly than the colourless. When a quantity of iodine, slightly more than sufficient to combine with the triphenylmethyl present in the yellow form, is added to a solution of triphenylmethyl in chloroform, a reddish-brown solution is obtained. The colour of the solution slowly changes, and when a sufficient quantity of the yellow variety has been formed, the solution has the pure yellow colour of the triphenylmethyl iodide.

The state of equilibrium,

$$\text{Triphenylmethyl (colourless)} \rightleftharpoons \text{Triphenylmethyl (coloured)},$$

is displaced towards the left by a lowering of temperature. A solution of triphenylmethyl in chloroform, which is orange-yellow at 15° C.,

becomes quite colourless at −63° C. The change in the colour intensity of a solution of triphenylmethyl iodide when similarly treated is barely perceptible.

This definite proof of the separate existence of two different triphenylmethyls strengthens the view first put forward by Gomberg, and later by Tschitschibabin, in the following words: "From the quinone theory of structure we must at least ascribe a quinone formula for triphenylmethyl in solution, while it seems that the hexaphenylethane formula is in general warranted for the colourless solid compound. In some cases a kind of Tautomerism can exist, so that the two isomers in the solid state have the same structure, while in the liquid and dissolved condition there is a mixture of both."

Numerous examples of the above equilibrium are to be found in both inorganic and organic chemistry, *e.g.*: the dimolecular colourless nitroso compounds and the unimolecular coloured molecules, of which Bamberger's tertiary nitrosobutane is typical: the latter in solution forms first colourless double molecules, which after a few minutes change into the blue coloured simple molecules.

The equilibrium of nitrogen peroxide, *viz.*:—

$$N_2O_4 \rightleftharpoons 2NO_2$$

serves as an excellent illustration of the two triphenylmethyls.

Wieland, criticising Gomberg's molecular weight determinations in which a double value for triphenylmethyl was obtained, states that the experiment must have been conducted in solutions whereby not more than about 5% of Schmidlin's yellow isomeride could have been present. In naphthalene, on the other hand, where the amount of the yellow unimolecular form is much larger, a much lower molecular weight was found.

In conclusion, it seems that while the final solution of this interesting problem is still for the future, yet every indication seems to point to the result that the colourless triphenylmethyl is really hexaphenylethane, while the coloured solutions contain the body implied by the term triphenylmethyl.

SOLAR DISTURBANCES DURING JULY, 1910.

By FRANK C. DENNETT.

DURING July a decrease was again noticed in the number of spot outbreaks. No dark spots were seen from the 19th until the 25th, inclusive, but faculae were recorded every day with the exception of the 20th. At noon on July 1st the longitude of the central meridian was 203° 13′.

No. 53 appears on this chart as it continued visible until July 12th.

No. 54.—On July 1st a penumbraless pore was seen in the morning, about half-a-dozen such formed a train on the 2nd, the number increasing during the day. It was less conspicuous on the 3rd, but on the 4th the leader had much enlarged, and also the eastern members, but to a lesser extent. Its greatest length was 33,000 miles, but it rapidly declined so that only one pore remained on the 6th, when it was last seen.

No. 55.—A solitary pore appeared on the 8th, and next morning a second one had developed, but only one remained until the afternoon.

No. 55a.—A pore was seen at first on the 11th, and about four developed later, though not seen after the 12th.

No. 56.—A solitary spot, the remains of No. 52, accompanied by faculae, was seen on the 12th, being over 15,000 miles in diameter. On the 14th the inner edge of the penumbra was brightly fringed, and also on the 15th, when the umbra had broken in two, then dwindled until the 18th, when last seen.

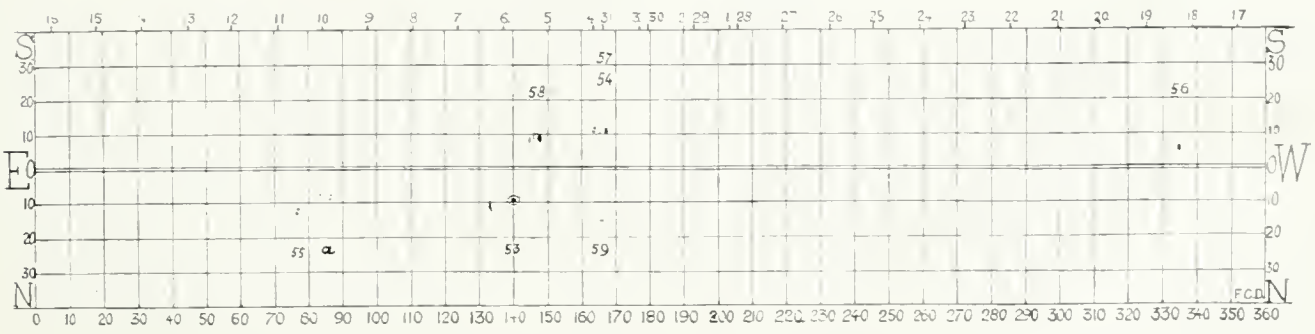
No. 57.—A tiny black pore in bright faculae was noticed on the 26th and 27th. On the 28th there were two pores west and one east, which increased on 29th. The eastern spot was largest on the 30th, forming, with other pores, a triangle with a base of 33,000 miles. On the 31st the western pores were almost gone, but the rear spotlet endured until August 2nd. The group almost exactly covered the site of No. 54.

No. 58.—On the 26th a group had come round, the leader being of fair size. They decreased, and altered in appearance very quickly, disappearing entirely during the 31st. The group was 30,000 miles in length.

No. 59.—A single pore only was visible on the 28th.

The chart has been constructed from the observations of John McHarg, A. A. Buss, E. E. Peacock and F. Dennett.

DAY OF JULY.



NEW METHODS IN ASTRONOMY

(AND THEIR APPLICATION TO THE EVOLUTION OF OUR SYSTEM).

By F. W. HENKEL, B.A., F.R.A.S.

DURING the last few years the researches of eminent British and Continental Astronomers have thrown a flood of light upon some of the most recondite problems of the science. Amongst these may be especially mentioned the work of Dr. Hill, M. Poincaré and Sir George Darwin, and we propose giving a simple account of some of their results, as well as the bearing these have upon theories as to the origin of our system. The Laplacian nebular hypothesis, greatly shaken by later discoveries and the critical investigation of its postulates, seems likely to undergo the fate prophesied for it by the late Mr. Proctor, founder of "KNOWLEDGE," and his words may be quoted here: "If ever proper enquiry is made into Laplace's nebular hypothesis, that also will be still more decisively rejected" (*Old and New Astronomy, p. 638.*) We shall have occasion to allude to some of the alternative hypotheses: an account of part of Professor See's work has already appeared in "KNOWLEDGE" (June 1909).

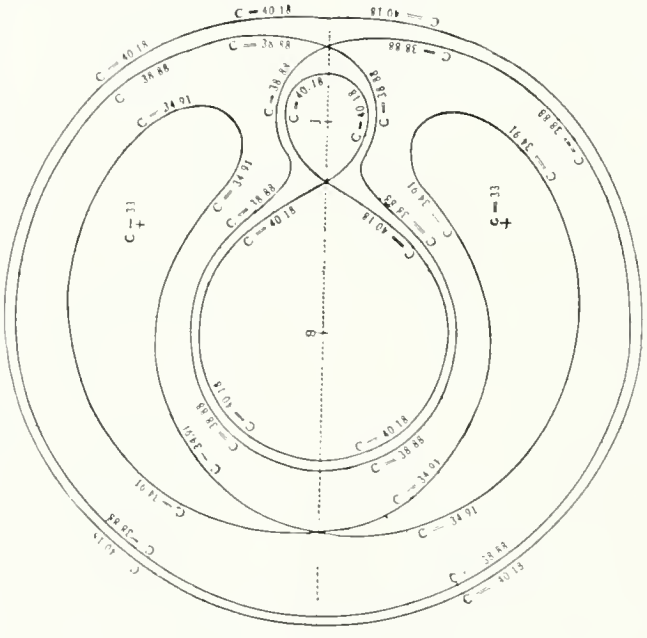


FIGURE 1. Curves of Zero Velocity (after Darwin).
Critical values only shown.

Newton, in his immortal Principia, has once for all solved the problem of two bodies. "Given two spherical masses acting on one another, according to the law of gravitation, and their position and motions at any time, to find their motions at any future or past time." The planets move in ellipses closely approximating to circles round the Sun, the comets mostly move in parabolas, a few in very elongated ellipses, and perhaps a very few in hyperbolas round the same centre of force. This, however, is only an approximation, though very close, to their motion, arising from the fact that the mass of the Sun is so enormously great compared with that of any of the planets, that the attraction between Sun and planet is the main factor in determining the path of any one of these bodies. Every planet moves in an orbit round the Sun which is very nearly the same as it would be were no other bodies but itself and the Sun to exist: the small deviations between the actual motion and this ideal motion are known as "perturbations," and are due to the disturbing action of the other members of the Solar system, arising from the difference of their attractions upon the Sun and the planet in question in each case. Owing to the fact that, as we have mentioned above, all the planets are much smaller than the Sun, we may, by a series of approximations, determine these perturbations one after another, and add their results till we obtain a satisfactory agreement between theory and observation. The general problem of *three* bodies is, however, far beyond the capacity of our present mathematics to solve, and all that can be done is to take some simple cases and then consider others approximating to them as nearly as possible.

The Moon moves in a path round the earth which is not very different from a circle, but, owing to the disturbing action of the Sun, the form of this path is continually undergoing slight variation. In this case, though the disturbing body is not *small*, yet on account of its much greater distance, the Sun's disturbing force or its *difference of action* upon Earth and Moon is only a small fraction of the Earth's attraction upon the Moon. Were the Sun to attract Earth and Moon equally and along parallel directions there would be no disturbance of their relative motion. In a similar way we may say that any external force acting equally upon all members of our system will produce no change in the planetary and satellite orbits. Whatever force may be moving our Sun along its mighty path towards "Hercules" (or Vega), is equally drawing its family of planets, satellites, comets and meteors. Even as it is, however, with the comparative simplification introduced by the smallness of the disturbing force, the "Lunar Theory" is one of the most complex branches of applied Mathematics, and though the *general* agreement between the results of theory and observation affords one of the most complete verifications of the exact truth of Newton's law of gravitation, there are questions even yet awaiting

solution. Dr. Hill, in 1876, pointed out a new method of treating the problems of the Lunar motion, and he took as the first approximation, not the ellipse, but what he called the variational curve as the Moon's orbit. The Moon is considered to move

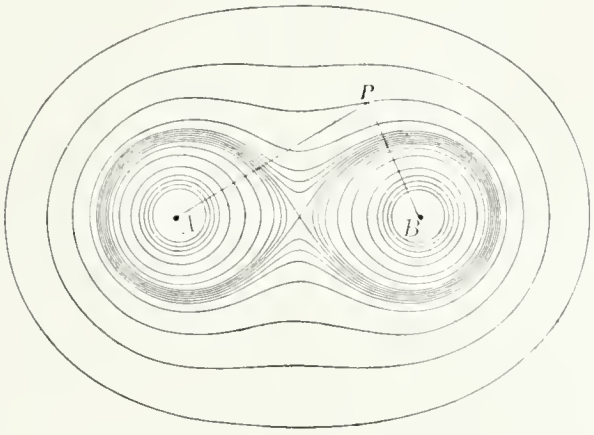
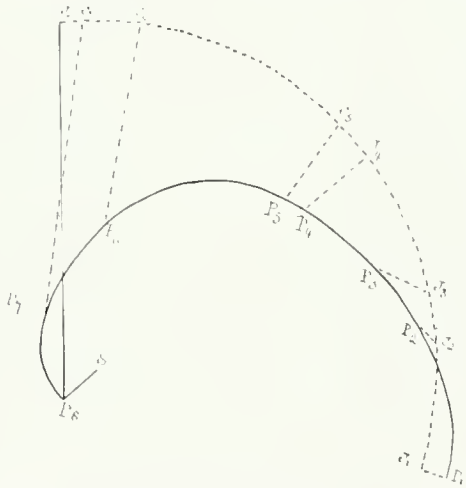


FIGURE 2. Equipotential surfaces about a double star with equal components of equal mass (after Darwin).

in a circle distorted by the Sun's attraction, and this orbit is called a "periodic one," its period being one synodic month (*i.e.*, interval from New Moon to New Moon or Full Moon to Full Moon, 29½ days). The eccentric form and the change of inclination (the Moon's path is not in the plane of the Earth's orbit but is inclined at an angle of about 5° to it) are "free" oscillations about the periodic orbit, the "annual equation" (arising from the varying distance of the Sun at different times of the year) is a "forced" oscillation. M. Poincaré, M. Gylden, and Dr. Charlier have continued this subject from the purely mathematical point of view, whilst Sir George Darwin has also dealt with it numerically in a *comparatively* simple manner, and we propose to give a brief account of some of his work. He begins by considering the case of three bodies, called respectively the Sun, a planet moving round it which he calls Jove, and a third body whose mass is infinitesimal (so that it will produce no appreciable effect upon the motions of the other two) which he calls the planet, or satellite. Jove (J), of unit mass, moves round the Sun, of mass* ten, in a circle, and all three bodies *lie in one and the same plane*. He first obtains what is called the equation of relative energy (the well-known Jacobian integral $V^2=2\Omega-C$). Since for real motion V^2 must be positive, 2Ω must be greater than, or at least equal to, C, and so the planet can never cross the curve represented by $2\Omega=C$, which agrees with Dr. Hill's result in assigning definite limits to the past and future position of the Moon in our system. He then considers the form of the curves obtained by giving different values to the constant C, called the constant of relative energy. For large values of C they are closed ovals round S and J respectively with an outer branch round both S and J. The larger oval, shrinking as C becomes less, unites with the inner oval round J, and the curve becomes of horse-shoe shape. The horse-shoe then narrows at the middle and breaks into two elongated portions, these gradually shrinking into two points equidistant from S and J. Figure 1 shows the form of the curves for the critical values.†

It was shown by Lagrange, more than a hundred years ago, that there is an exact solution of the problem of three bodies when they are each at one angle of an equilateral triangle which revolves uniformly, and since two triangles of equal altitude may be drawn on the same base (one above and the other below S J), there are two such positions. This is approximately realised in our system. A minor planet recently discovered revolves at about the distance of Jupiter from the Sun, and this arrangement appears to be a stable one. A paper on this relation appeared



A. Orbit of Satellite leaving Jove and passing under the control of the Sun.
(Thick line, orbit of Satellite. Path of Jove, dotted line).

B. Referred to moving axes.

FIGURE 3. (After Darwin).

some time since in "KNOWLEDGE." In addition to the above determinations, Sir George Darwin has also considered the case of a satellite leaving Jove and passing under the control of the Sun, and Figure 3 shows the path for C=39, referred both to axes fixed in space and to revolving axes. The full curve shows the path of the planet, the dotted

* The value of ten for the mass of the Sun in terms of Jove is taken for the purpose of exaggerating all the phenomena of perturbation as compared with those in our system, so as to give a clearer view of their nature.

† We may regard these curves as the traces on the plane of the paper of *surfaces* of zero velocity. By taking all three bodies in one plane, the problem is reduced to a two dimensional one.

one is that of Jove, and lines are drawn joining the simultaneous positions of these bodies. (In order not to complicate the figure only a few of these are drawn.) The body here makes a single revolution round Jove, and is then drawn off to the Sun, but it may happen that several revolutions round the former would be made until conjunction takes place at "apojove" (greatest distance of planet and Jove,) and then it will revert to the Sun again. "It seems likely that a body of this kind would in course of time find itself in every part of the space in which its motion is confined" (Darwin). Thus it would at last collide either with the Sun or Jove, and it has been suggested by the same writer that in this way stray bodies are gradually swept up by Sun and planets. Some non-periodic orbits passing from Jove to the Sun are drawn in Figure 4, and it will be seen that one of these paths has a sharp cusp (lying on the curve of zero velocity), another is looped, and so on.

Regions within which stable orbits are possible, and others for which they cannot exist, are found, and in this connection we may later find an explanation of Bode's Law as to the distances of the planet and satellites from their primaries. In his later work, Sir George Darwin has considered possible retrograde orbits, all his former researches having been on orbits where the motion is direct, *i.e.*, the same direction as that of all the planets and most of the satellites, and has traced the forms of paths of bodies ejected from Sun towards Jove and *vice versâ*. Modes of transition between direct and retrograde orbits are also considered, passage through the apices of the equilateral triangle on S J as base already mentioned being suggested as one method. From a consideration of the figures given we may gain much information as to the kind of orbits possible, and the limit of distance between planets and satellites. For values of C greater than 40·18 the third body must be either a superior planet moving outside the larger curve, or an inferior planet moving inside the larger internal oval, or a satellite inside the smaller oval, and it can never change from one of these forms to either of the others, "so that this limiting value gives superior limits to the radii vectores of inferior planets and of satellites which cannot sever their connection with their primaries." When C is less than 40·18, and greater than 38·88, the third body may be a superior planet, or an inferior planet, or satellite, or a body moving in an orbit partaking of the two latter characteristics, but it cannot exchange from the first *rôle* into either of the others. When C is less than 38·88, and more than 34·91, the body may move anywhere except inside the "horse-shoe" shaped space. Here the distinction between planetary and satellite motion does not exist, and the body may change from any one of these modes into any other. When C lies between 34·91 and 33, the only regions where motion is not possible are within the leaf-like folds on each side of the line S J, and these diminish to the pair of points marked with crosses (Figure 1), for the value C = 33. Lastly when C is less than 33 the body may move anywhere. As a general result it is concluded that unstable orbits are such as to lead to the ultimate absorption of bodies moving in them into one or other of the larger bodies, and though the problem in our planetary system is much more complicated from the large number of planetary bodies, yet, as we have seen, several interesting features have indicated explanations. We have now to briefly notice the bearing of these results upon cosmogonic theory, and some modifications suggested by Professor See. By supposing the existence of a resisting medium universally diffused throughout our system, of whose presence we *have* some evidence, we get the well-known result that when a body revolves about the Sun or a planet, the resisting medium at every point decreases the velocity, and thus gives the central attraction more effect, the revolving body being gradually drawn nearer to the centre of force. For Encke's comet this change *seems* more rapid than for any other known member of our system. For orbits differing from circles, the result of the action is also to diminish the *eccentricity*, or make them more nearly circular. This was well known to Laplace, and the proof is given in works on analytical dynamics. Thus a particle moving within one of the inner closed surfaces, of which we may regard the curves (shown in Figure 1) to be the traces on the plane of paper, will gradually draw nearer its centre, and, its speed increasing, its periodic time will be diminished: for a body moving within the "hour glass" shaped space or the pear shaped region surrounding both bodies (Figure 1) will drop nearer and nearer to one or other of the masses, and may finally pass within the closed surface and acquire a constant of relative energy such that it can never escape again from that region. The Moon is now within such a closed surface about the Earth, and cannot escape therefrom, and it has been accordingly supposed that it has not at any time been outside: but if we admit the action of the resisting medium there is a *possibility* of our Moon's having been an independent planet which approached too nearly to the earth and became captured. At present most astronomers give in their adhesion to Sir George Darwin's theory of

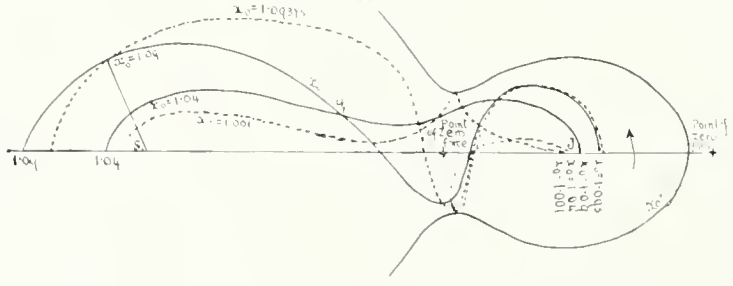


FIGURE 4. Non-periodic orbits passing from Jove to the Sun (after Darwin).
Outer thick line curve of velocity C = 39·0.

tidal evolution, according to which our Moon once formed part of the earth's substance. It is supposed that once upon a time the latter was rotating much more quickly than at present, and that a portion separated from the rest and became the moon. The time of the earth's rotation was estimated to be then about two and a half hours, so that the "centrifugal force" caused its rupture. The separated portion may have gradually receded to its present position by tidal action. Difficulties as to the possibilities of the Moon's holding together under tidal strain when so much closer to the earth than at present, and the cause of the supposed very rapid rotation of the earth in early ages, have been raised by various writers, so that we may at least regard the alternative capture theory and action of the resisting medium as a possible explanation, though it seems difficult to imagine that all the relations pointed out by Sir George Darwin are purely accidental. The Laplacian hypothesis, with the modifications suggested by Faye and others and supplemented by the theory of tidal evolution, has been nevertheless shown to be untenable, and the evolution of the planets by separation of rings of matter from the central condensation as it contracted must be definitely abandoned. Briefly stated the theory is as follows. The matter now forming Sun, Earth and other planets was at one time in the form of a hot gas, and was of approximately spherical form. It rotated slowly on its axis, the rotation increasing in swiftness as it grew colder and contracted. In time rings of matter were left behind. Each of these rings gradually collected into a single globe, and thus the planets came into existence. A planet thus formed continuing to revolve itself, in turn abandoned rings in contracting, and thus arose the satellites. This process is illustrated by a well-known experiment, devised by Plateau. He formed a mixture of alcohol and water of the same density as oil (water being heavier, alcohol lighter), and poured into this mixture a quantity of oil. The oil sank about halfway and floated in the middle of the mixture as a round ball. By means of a disc attached to a wire, Plateau made it rotate. The rotation caused the ball to spread out into the form of a spheroid; then, as the speed increased, a ring was thrown off which revolved round it. The ring broke up after a time and collected into a smaller globe, which rotated and also revolved round the larger; then another ring was thrown off as the speed again increased, and so on. Babinet, however, in 1861 showed that the original nebula imagined by Laplace could not have rotated with sufficient speed to detach any of the planetary masses, and his argument has been extended by Professor See to show that, similarly, the satellites could not have separated from their primaries. There has therefore been proposed an alternative fission theory, that secondary condensation nuclei might be formed by *gravitational* instability within the gaseous nebula, as also the capture theory referred to above, together with the "spiral" nebula form. A spiral nebula is supposed to be formed from a Sun by the near passage of another star, the consequent ejection of matter forming two spiral arms bearing nuclei at irregular intervals. The particles of these arms move round the central nucleus in elliptic orbits, the planets being formed by the gradual growth of these nuclei, by the addition of small "planetesimals." Professor See considers that our solar system was formed from a spiral nebula and that the planets have not been detached from the central mass by rotation, but "captured," or added on, from the outer parts of the nebula, their orbits have been "rounded off" by the resisting action of the medium in which they move, and the particles moving in orbits confined to regions of the nature of those indicated in the researches of Darwin alluded to in the earlier part of the paper. Moving against resistance they will all drop nearer to one or other centre of attraction: some will become satellites of one planet, some of another, whilst yet others will collide with and be absorbed by these latter or fall upon the Sun. Thus the greater masses will increase, and in some such manner the "secular acceleration" of the Sun and the unexplained acceleration of Mercury, may be explained. In our system we have a large central mass, the Sun, with attendant planets, but in some cases we may have a more nearly equal division of the matter, as in the double stars. Figure 2 shows the equipotential surfaces about a double star with equal components (or rather their traces on the plane of the paper). In such cases the planets which are developed, if they can continue revolving in stable orbits, will have to be near either one or other of the large masses, or else at great distances from both, if their motion is to be of a permanent character. However, the existence of such bodies, though probable, is not likely to be revealed by any increase in telescopic power of our present instruments. Great numbers of spiral nebulae are now known to exist, scattered

The Spiral Nebula M. 51 Canum Venaticorum (after See).

Illustrating the Spiral Theory.

all over the heavens, whilst there are very few of the oblate spheroidal form, such as the hypothesis of Laplace assumed to be met with in the sky. This was pointed out by Proctor and Ranyard (*Old and New Astronomy* § 1445). Such nebulae as we see have a greater analogy with the solar corona than with the fiery condensing mist conceived of by Laplace. The opposite branches often seen on photographs of spiral nebulae, are considered to represent the "original streams of cosmical dust coiling up and forming spiral systems." If the streams converge and the mass becomes very dense at the centre, a double star may be formed.

The theory of the action of a resisting medium gives results exactly the opposite of tidal friction. The latter usually *increases* the major axis and eccentricity of a planetary orbit: the resisting medium decreases both these elements. "In the actual universe both causes are at work, sometimes one predominating, sometimes the other."

In the growth and development of our system all these various agencies may have been called into play, so true is it that here, as elsewhere, no single explanation is self comprehensive and exclusive of other hypotheses. The mind of man is so constituted that it cannot help speculating on such topics, but it is well to remember that all our knowledge, both of time and space, is essentially relative and conditioned: no absolutely undisputed truth is admitted by all philosophers alike, so that in some respects we *seem* less advanced than was thought to be the case a century ago. The Sun was supposed to have an *absolute* velocity of about fifteen miles per second "towards a point in the constellation of Hercules," but, as has been pointed out, this result has been overthrown by the work of Kapteyn. Numerous optical and electrical experiments have been made at different times with the view of determining the absolute velocity of the Earth, but such effects have always failed to appear.

"The vast masses of observational data accumulated by the persevering industry of self-denying men of science," together with the mathematical researches we have outlined above, are leading to more accurate views than any that have been held hitherto, and the results already obtained should give us encouragement in our efforts to trace "the process of Creation," but we must more than ever feel how little after all is yet known compared with what remains to be learned.

QUERIES AND ANSWERS.

Readers are invited to send in Questions and to answer the Queries which are printed on this page.

QUESTIONS.

2 and 6 still remain unanswered. (See pages 288 and 301).

7. SCIENTIFIC IDEAS OF TO-DAY.—In Mr. Gibson's instructive book, *Scientific Ideas of To-day*, I find the following statement, on p. 207:—

"Picturing the electron revolving around its atom and setting up other waves of definite length (definite colour), we can imagine readily that if the atom itself were caused to vibrate at an abnormal speed, the revolutions of the electron might be very materially affected. We have evidence of such occurrences. For instance, a certain chemical compound—the double iodide of mercury and silver—appears yellow at normal temperatures. If we hasten the vibrations of its atoms by increasing the temperature to about one hundred-and-ten degrees Fahrenheit the substance appears red."

Again, on page 157 we read:—

"By applying heat to a body we hasten the speed of these revolving electrons. . . . In a lump of iron at ordinary temperature the motion of these electrons is disturbed by molecular collisions, so that the resulting rates of revolution cause very long waves in the aether. . . . To return to the very simple experiment of striking a piece of iron by a heavy hammer, we are able, by repeated blows, to cause the molecules to vibrate more rapidly, and this enables these revolving electrons to quicken their pace."

Now if by means of heat the revolving electrons of the chemical compound are forced to quicken their pace, why should the other waves produced be "red"? That is a colour produced by the electrons revolving at a slower pace than those producing the original colour, yellow. From the above statement I should have expected that the colour produced by heating the compound would rather have been higher up the colour scale than the yellow. "PERPLEXED."

8. HAIR COLOURS.—There are cases where the hair at birth is dark coloured or almost black in the case of human babies. When the child is four years old or so the hair becomes bright golden, while in adult life a shade of colour intermediate between the two previous ones is acquired. Has anyone studied this question, and has any theory been brought forward as to the colours having been inherited from any particular ancestral races (say in the case of English people) as are the spots on young lions and tapirs?

R. R.

REPLIES.

4. For the information of Mr. Sandeman it may be pointed out that queen wasps are the only ones that survive the winter. Hence it stands to reason that they must be fertilised before hibernation, and that they must fulfil the duties of founding a new nest and rearing the first larvae.

"BIOLOGIST."

3. In reply to Mr. Jeffery's enquiry with regard to Barometer readings, my Barograph, which is set $\frac{1}{10}$th higher than the Standard at Victoria Park, Portsmouth, registered at 10 p.m. on the 26th of January, 1905, 30, $\frac{9}{10}$ths.

At 9 a.m. on the 27th, it registered 31 (7 degrees of frost during the night) and at 9 a.m. on the 29th, it registered 31 and $\frac{1}{10}$th. This is the highest reading my Barograph has ever recorded in my experience of about eight years. It is set accurately for its height above the sea level, but seems to differ $\frac{1}{10}$th from the Standard at Portsmouth.

Harmsworth's Encyclopaedia states (Art. Barometer) the highest pressure ever observed was 31·717 at Irkutsk, 20th December, 1896, and in the British Islands at Ochtertyre, Perthshire on the morning of 9th January, 1896, 31·108. The Royal Observatory at Greenwich would probably be the best authority on this subject.

JOHN GLAS. SANDEMAN.

ANIMAL HEAT.

By DAVID FRASER HARRIS, B.Sc. (Lond.)

Of the University, Birmingham.

One of the most noticeable features of an animal is that it can produce heat. Biologists long ago came to the conclusion that it was by the oxidation chiefly of the carbon and hydrogen in or by the living substance that animal heat was generated. By "long ago," we mean the time of the great chemist Lavoisier, who, though he made some mistakes in animal chemistry, certainly understood that animal heat and the heat of combustion were alike in this that they were both due to oxidations. Before the discovery of oxygen by Priestley and Lavoisier, this position could not have been attained to, although both Lower and Mayow of Oxford by 1669 had arrived at the conclusion that there was something in the atmosphere (Mayow called it "nitro-aërial particles") which was essential for the maintenance of animal life, and for all sorts of combustions. This was the dawn; Priestley and Lavoisier were morning stars; we live to-day in the high noon of the understanding of animal heat.

The chemical view of the origin of animal heat was, however, lost sight of by two such eminent thinkers of the eighteenth century as Boerhaave and Hales, who held that the heat of the body was due to the friction between the blood-corpuscles and the blood, and between them and the walls of the vessels. This is known as the iatro-physical view of animal heat. One can only mention the famous view of Stahl (1687), that the "principle of fire was phlogiston," a Teutonic "will-o'-the-wisp" that retarded the advance of thermo- and bio-chemistry for quite a hundred years.

But if by "long ago" we mean the centuries before the Christian era, we shall find that Aristotle, for instance, who seems to have had something to say on everything in heaven and earth, taught that animal heat was something absolutely unique. Aristotle believed that animal heat was something *sui generis*, something of an absolutely different order from heat of non-vital origin—the heat of a fire, or of a gas compressed, or of friction, for instance. He thought that the ultimate source of bodily heat was an eternal essence, the source of being of heaven and the stars, which latter were imagined as intelligent existences.

This was, of course, the foundation of astrology, the notion that these celestial intelligences could interfere in human affairs, and were capable through the harmony of their movements of creating music—the "music of the spheres."

For long after Aristotle's time the idea prevailed that the blood, recuperated by the food in the intestines, was kept fluid in the heart by the heat there; but that the heart, becoming too hot, had to have cold air sucked in by the lungs to cool it. This teaching actually lingered until the time of Harvey who died in 1657.

Animal heat, as distinguished from every other kind, was also thought to have quite special curative powers. Thus we are told in the Book of Kings that when King David "was old and stricken in years. . . . they covered him with clothes but he gat no heat." His servants recommended that a young girl should be brought to impart heat to him; "let her lie in thy bosom that my Lord the King may get heat."

Our present view is that one of the attributes of living matter is to oxidise carbon and hydrogen to form carbon dioxide and water, which process cannot be effected without the evolution of a certain amount of heat. Whether or not we are able to detect the heat so produced is quite another matter. One of Lavoisier's original ideas was that all the animal heat was produced in the blood of the lungs; that the lungs were the furnace of the body, and that the heat was carried thence by the circulating blood. We now know that the blood is responsible for the production of very little heat indeed, and are quite certain that it is in the muscles that by far the greatest amount of bodily heat is manufactured. That a bloodless muscle could produce heat even after it was cut out of the body was proved for the first time by Helmholtz in 1848.

Everyone is familiar with the distinction between cold-blooded and warm-blooded animals, but this classification has lost its meaning. We now speak of animals which are able to maintain a constant temperature and of those which are unable to do so, and these answer to the old warm-and cold-blooded animals respectively. Those animals whose body-temperature rises and falls as the temperature of their environment rises and falls are now known as poikilothermic, and those which are able to maintain a constant temperature, however much the temperature of the environment rises or falls, are known as homoiothermic animals. This new classification has been found necessary because of facts such as the following:—The temperature inside a hive of bees (and insects are classed as "cold-blooded) may even in winter time rise 21° C. above that of the outer air; a mammal (a "warm-blooded" animal) has, when hibernating, a temperature almost the same as that of its cold environment: the temperature of a newly-born mouse goes up and down with that of its surroundings; a frog or fish placed in warm water becomes as warm as the water. In

other words a cold-blooded animal can, under certain circumstances, become warm-blooded, and a warm-blooded animal can become cold-blooded, so that all seems confusion.

The important point is not whether the animal feels hot or cold to human touch at any given moment of observation, but whether the animal can or can not maintain a constant temperature while the temperature of its environment alters.

In health, birds and mammals are able to keep their body temperature the same, no matter how high or how low (within limits) the temperature of the environment goes. The temperature of all other creatures rises with a rise and falls with a fall of the outside temperature. In this latter class we must include the human infant, hibernating mammals and newly-hatched birds.

The power possessed by all living things, plants included, to produce heat as one of the manifestations of their vitality is referred to as thermogenesis. No doubt the actual amounts of heat produced by different animals are very different: a fish in unit time per unit of its weight is producing vastly less heat than a man, and a man less than a bird; but they are all producing heat. The thermogenic centres are chiefly muscles, including the heart, the liver and the glands; these, and not the blood, are the thermogenetic foci. A frog whose blood has been replaced by a weak solution of common salt still continues to excrete carbon dioxide for some days, showing that oxidations are still going on in it although it has no blood at all. It is just as certain that an animal producing carbon dioxide has been and is producing heat, as that a candle which is liberating carbon dioxide is producing its heat through the oxidation of the carbon of its fat.

Before we go further into the subject of animal heat, it would be well to understand the difference between temperature and quantity of heat. A difference of temperature between two bodies, A and B, is the expression of the fact that heat is leaving A for B or vice versa. If two bodies are at the same temperature, no heat will pass from the one to the other. Temperature is of course usually measured by the visible expansion of some substance, mercury or alcohol, which can take in heat from a hotter body, or on the other hand allow heat to leave it for a colder one. Temperature is to heat what potential is to electricity. Temperature tells us nothing about quantity of heat, but merely whether heat is or is not leaving a given body, and, in a certain sense, how fast it is leaving it. Several things all of the same temperature may, however, feel very different to us on our handling them; some will feel warm, some cold. For instance the marble mantelpiece and a "Tweed" coat may be at the same temperature as tested by a thermometer, yet the former feels cold compared with the latter. The chief reason is that marble is a good conductor of heat compared with the coat, and abstracts from our skin so much more heat in a given time than does the coat. Thus, getting in between linen sheets is so much "colder" than getting into blankets, because linen is so much better a conductor of heat than is wool.

Measurement of the quantity of heat is effected by an instrument known as the calorimeter. The principle of this is that the heat produced by any particular substance is all absorbed by a known weight of water, which is, in consequence, raised a certain number of degrees in temperature. A calorie, or unit of heat, is defined as the quantity of heat transferred to one kilogramme of water (2·2-lbs.) in order to raise it one degree Centigrade in temperature. A water-calorimeter is, then, essentially a metal box immersed in a known quantity of water. To ascertain how much heat an animal gives out in a certain time, it is only necessary to place it in the inner box, which is of course properly ventilated. The heat that radiates from its skin is conducted through the box to the water; the heat of its expired air is absorbed by the water, the temperature of which, we shall say, has risen n degrees. We shall suppose that the animal has done no external work while it has been in the calorimeter. If the weight of water is W units, then $W \times n$ is equal to the calories of heat lost by the animal in a given time.

The problem might now be faced, from what materials does the body manufacture heat? Since the body only very exceptionally absorbs any heat from outside sources—heat of the Tropics or Turkish bath—it is quite clear that it must be constantly taking in the wherewithal to manufacture heat so continuously. Except in starving animals, the source of heat is the oxidation of the food.

Just as a steam-engine transforms the potential chemical energy of the coal into the kinetic forms of energy—heat and external work (movement)—so the animal body transforms the potential energy of the food into the active forms of heat and muscular movement. Both are machines for transforming energy; the one is of iron, the other of protoplasm, but in both equally is the great law of the Conservation of Energy obeyed. This may be said to have been one of the mathematico-physical intuitions of that great mathematico-physical physiologist, Helmholtz. For a long time it was thought that the animal organism was outside the pale of that great generalisation, but it is now known that an animal in a calorimeter will produce as many calories of heat in a given time as would have been generated by the burning of a weight of food equal to that of the food digested during the period in question. It is only a question of difference of rate in the setting free of the heat: in burning it we get all the heat out at once; in digesting it we get out just as much heat, but it is liberated much more slowly. The inhabitants of cold countries are prë-eminently fat-eaters: the notion that fats give heat and sugars energy is probably not altogether fallacious.

The actual amount of heat that each adult loses per twenty-four hours is, in round numbers, about three thousand calories, the ultimate source of which is the oxidation of the various oxidisable chemical

elements in the food of twenty-four hours. Knowing that one gramme of protein (flesh) yields us 4·1 calories, one gramme of sugar 4·1, and one gramme of fat 9, it is not difficult to construct a dietary containing potentially the necessary heat. We have further to know that the ratio of the weight of nitrogen-containing to non-nitrogen-containing substances must be about 1 to 3·5 or 4·5.

The portions of the total heat-loss borne by each system have been determined as follows:—

Heat radiated and conducted from the skin, 2190 calories or 73%.
Heat lost in evaporating water from the skin, 435 calories or 14·5%.
Heat lost in evaporating water from the lungs, 216 calories or 7·2%.
Heat lost in warming the expired air to body-temperature, 105 calories or 3·5%.
Heat lost in warming the dejecta, 54 calories or 1·8%.

In human beings, then, the skin is *the* system of heat-loss, being responsible for the loss of 87·5% of the total heat lost. This is not so in some animals: the dog, for instance, whose hairy coat does not permit its skin to perspire. The dog loses heat largely by its expired air, and by the radiation and evaporation of water from its tongue: thus it pants and puts out its tongue on a hot day.

Obviously our clothes prevent the loss of heat, and the more effectually as they are bad conductors of heat. For this reason flannel, wool and furs are so much "warmer" than linen or cotton, materials from the vegetable kingdom, and therefore not the natural clothing of animals.

An animal is not only a transformer of energy, but it is the most economical transformer known: for whereas in the very best steam-engine we obtain about twelve per cent of the original potential energy as motion (external work), in the animal we get as much as twenty-five per cent in the form of useful work. And whereas in the engine a great deal of the heat set free is lost or wasted from the engineer's point of view, the animal heat, so far from being wasted, is all essential to the protoplasm to provide it with the optimum temperature for the performance of its vital activities.

The muscles as energy transformers differ from an engine in possessing two distinct powers, heat-producing and work-producing, or the thermogenic and dynamogenic respectively. Now the interesting thing is that, although a muscle cannot do work without producing heat, it can continue to produce heat without actively contracting, without doing work. In this latter state it is said to be in a condition of tonus or tone. No doubt when sufficiently analysed tonus is discovered to be a state of imperceptible or incipient contraction. When a muscle having shortened continues to support a weight but shortens no more, it continues to produce heat although in the sense of the physicists it is not doing any external work: thus dynamogenesis and thermogenesis are separate capabilities. But further, the two related capacities of muscle for heat- and work-production only vary *pari passu* within certain limits. As a muscle lifts heavier and heavier weights it sets free more and more heat each time, and also if it raises the same weight each time, but under the influence of increasingly strong stimuli, it will evolve more and more heat each time. In this latter case the dynamogenic effects are the same throughout the series, while the thermogenic effects are greater and greater. As fatigue comes on, the heat-producing faculty is impaired long before the work-producing. Thus very tired muscles may be able to lift the load to the height to which they raised it when fresh, but they now do so with the greatest possible economy as regards their store of potential energy, for they evolve the minimal quantity of heat. They oxidise material to as slight a degree as possible. Muscular work and heat are thus not quantitatively parallel products: there is nothing comparable with this in a machine of human construction.

As regards the kinds of food from which we derive heat, our ideas have undergone changes since the time when the great chemist Liebig divided foods into flesh-formers and heat-givers: the "meaty" stuffs and cheese he placed in the former group, the starches, sugars and fats in the latter. After having been much criticised, this classification by Liebig is admitted to be substantially correct. While sugars and fats do not build up tissue, but are found to be normally oxidised more or less directly for heat-giving or work-producing purposes, the proteins (flesh-formers) both repair tissue-waste (by their nitrogen-containing moiety), and also contribute to heat-production by the non-nitrogen-containing substances into which in digestion they are split up. Liebig told the truth but not the whole truth.

If during a certain time a person's temperature remains constant, that person is losing as much heat as he is producing. In technical language, thermogenesis, or heat-production, is just balanced by thermolysis, or heat-loss. Obviously, since we are constantly producing heat, unless we were as constantly to lose it, we should have to get hotter and hotter, and would damage our tissues by fatal fever. Now, it is very well known that the temperature of a healthy man (98·4° F. 37° C) hardly differs by a degree from one year's end to the other, or from the poles to the equator. If no heat were lost at all, our temperature would rise one degree Centigrade in half an hour; in thirty-six hours our body fluids would be boiling: in another thirty-six autocremation would be far advanced.

We may now ask ourselves the question, how it comes about that some animals have and others have not the power to keep their body temperature constant. Does not a fall of temperature necessarily depress vitality, and a rise, within limits, exalt it? The answer is that protoplasm *quâ* protoplasm is certainly depressed by a fall and stimulated by a rise of temperature, as can be well seen by the slowing effect of chilling an isolated frog-heart and the accelerating effect of warming it. It would seem then that there ought to be no such

things as animals of constant temperature (warm-blooded). The solution to the puzzle is the high development of the nervous system in animals known as homoiothermic. In a mammal when the temperature falls, the first result is loss of heat from the skin and an incipient depression of the temperature, but, through the nerves of the skin, impulses are sent into the nervous system which emerge as reflex stimuli to cause contraction of the skin vessels and increased heat-production in the muscles. The constriction of the skin vessels shuts some blood out of the skin and so diminishes heat-loss, while the increased tone of the muscles increases heat-production, and thus the loss of heat due to the fall of temperature is so rapidly compensated for that the temperature does not finally fall. Shivering is the familiar reflex effort on the part of the muscles to increase their heat as a result of the superficial heat-loss.

This power of balancing thermogenesis against thermolysis we call thermotaxis: the homoiothermic animals have, while the poikilothermic animals have not, the power of thermotaxis. Now fever (pyrexia) is the upsetting of thermotaxis, the disturbance of this beautiful thermic balance. Theoretically, fever, or a rise of temperature, may occur if the heat-production is too rapid for the heat-loss, or on the other hand the heat-production being unaltered, if the heat-loss is dimished in rate. Both these states of the upset thermal balance occur. Dr. Hale White, the London physiologist, has shown that whereas in pneumonia and erysipelas the fever is due to increased thermogenesis, in typhoid fever and in suppuration the rise of temperature is due to a diminution in the thermolysis. Fever is to-day regarded by physicians in a totally different light from what it was even a few years ago—in itself a wholly bad thing to be reduced at any cost. The increased heat-production is looked on as a reaction on the part of the living cells to the noxious stimulus of the micro-organism or its soluble poison, a response of a protective nature rather than of any other kind. Hence the indiscriminate lowering of the temperature by drugs (antipyretics) is not now nearly so common as it used to be. It is recognised as possible that the increase of heat (fever) may be evidence of sufficient vitality on the part of the living protoplasm to withstand the assaults of the infective agents, the increased heat being the bio-physical response to the micro-organic insults. The drugs which benefit fever most are now regarded as doing so, not because they lower the temperature, but because they attack the specific cause of the malady; quinine, for instance, in malaria destroying the parasitic plasmodium malariae, the salicylates antagonising the materies morbi of rheumatic fever. Those versed in vegetable physiology have been able to show that even in the case of parasites attacking trees there is a rise of temperature as a reaction to these assaults; and therefore botanists actually speak of "fever" in plants.

Of course it is not to be imagined that under no circumstances is fever, or very hot blood, injurious to the body. Within the last few years definite experiments have been made showing that blood hotter than a certain temperature *does* permanently damage the cells of the central nervous system. A short exposure to 47° C, or a longer one to 42° C, killed the cells of an animal's brain by coagulating one of the essential constituents of their living substance. "Sunstroke," as it is called, is the result of the too hot blood injuring the cells of the brain, and especially those related to consciousness. When the cells are only slightly injured the person may recover, and be "a little queer in the head" for the rest of his life; if, however, the cells are decidedly over-heated, as in "heat-stroke" or "heat-apoplexy," death in collapse supervenes, the person never regaining consciousness. Heat-stroke of this kind may occur in places to which the sun never gains access, as, for instance, in front of the furnaces of a steamer in the Red Sea. On the other hand, depression of the temperature of the blood below its normal is as fatal, although not so rapidly, and for quite other reasons. Great loss of heat depresses the tissues so that death results. What is known to Coroners' juries as "death from exposure" is really death due to heat-loss. An underfed, poorly clothed, and perhaps also alcohol-intoxicated person falls asleep out-of-doors on a frosty night; so much heat is lost that the heart and nervous system never recover: the person never wakes. The lowest (subnormal) temperatures in man have been recorded under these conditions (about 80° F).

The fact of the constancy of the temperature of the blood was recognised in the graduating of the thermometer. Very early in the evolution of that simple but indispensable instrument, an upper and a lower fixed point had to be agreed on. The lower point was virtually fixed in 1665 when Hooke advised that the temperature of water in the act of freezing should be accepted as the zero. Halley, the astronomer, was one of the first to assert that the temperature of boiling water was always the same, and thus a second fixed point was got. It is believed that the suggestion that the temperature of the blood or "blood-heat" might be a convenient fixed point was made by no less a man than Sir Isaac Newton in 1701. The temperature of the armpit of a healthy man, Newton proposed to call twelve degrees, the temperature of freezing water being zero. Fahrenheit in 1714 adopted this suggestion and took the body temperature as a fixed point, but his scale had 180 degrees in it, for it ranged from −90° (temperature of a mixture of ice and salt) to +90°, which was blood-heat. Acting on the suggestion of the Danish astronomer Roemer, Fahrenheit call his zero (which he thought absolute zero) 0°, and blood-heat 24°, a duodecimal not a decimal scale. Later he became dissatisfied with the largeness of the degrees, and therefore subdivided each into four: twenty-four thus became ninety-six, which accounts for a figure of this order standing for the temperature of the blood, a fact which very few people could satisfactorily explain.

ON THE STUDY OF DOUBLE STARS BY AMATEUR OBSERVERS. II.

By G. F. CHAMBERS, F.R.A.S.

THE LABOURS OF THE HERSCHELS.

THOUGH the name "Struve" occurs so unceasingly in the following pages, it must never be forgotten that it was Sir W. Herschel who must be regarded as the "father" of Double Star Astronomy, and, considering the epoch at which he lived, his labours in this field, and their results, deserve our highest admiration. It was Herschel who made the telescopes without which Double Star Astronomy could never have grown at the beginning of the 19th century as it did: it was he who developed their number from a dozen to hundreds which, later on, became thousands; and, more remarkable still, it was he who first suggested that the Newtonian Law of Gravitation was Universal in a sense never before dreamt of. That is to say, that it applied not simply to our relatively puny Solar system, but to systems of celestial bodies distant from us untold and untellable millions and millions of miles.

The story of Herschel's investigations as to Double Stars has never been fully told, and it would lead me too far afield to deal with it here in the detail which it deserves. The matter was handled by the late Miss Agnes Clerke in her customary excellent style in her book, *The Herschels and Modern Astronomy*, to which I refer the reader, merely giving here a very bare outline of a few prominent facts.

Though Sir W. Herschel was not only the first* but one of the greatest of Double-Star observers, he became such by "drifting" into the subject, rather than from any set purpose of studying Double Stars in the abstract. One thing led to another, and his discovery of Uranus and general telescopic work encouraged in him a desire to obtain a further insight into the constitution of the heavens. Parallax of the Stars was his first particular point: and he thought that the study of Double Stars would start him on the road to success in the determination of stellar parallax. His first Catalogue of 269 Double-Stars was completed in 1781, and was published in the following year in the *Philosophical Transactions*.† A second Catalogue followed in 1785,‡ but a long interval elapsed before the third, which did not appear till 1822,§ though some additional notes, not in the shape of a formal Catalogue, appeared in the *Philosophical Transactions* in 1803 and 1804.¶

The principle of the Double Star method of seeking to ascertain the parallax of a star is thus succinctly stated by Miss Clerke: — "It is perfectly simple, depending upon the perspective shifting, to a spectator in motion, of objects at different distances from him. In order to apprehend it, one need only walk up and down before a lamp placed in the middle of a room, watching its apparent change of position relative to another lamp at the end of the same room. Just in the same way a star observed from opposite sides of the Earth's orbit is sometimes found to alter its situation very slightly by comparison with another star close to it in the sky, but indefinitely remote from it in Space. Half the small oscillation thus executed is called that star's 'annual parallax.' It represents the minute angle under which the radius of the terrestrial orbit would appear at the star's actual distance. So vast, however, is the scale of the Universe, that this tell-tale swing to and fro is, for the most part, imperceptible, even with modern appliances, and was entirely inaccessible to Herschel's observations. Yet they did not remain barren of results."**

Writing in 1781 Herschel said that:—"As soon as I was fully satisfied that in the investigation of parallax the method of Double Stars would have many advantages above any other, it became necessary to look out for proper stars. This introduced a new series of observations. I resolved to examine every star in the heavens with the utmost attention and a very high power, that I might collect such materials for this research as would enable me to fix my observations upon those that would best answer my end.

"The subject has already proved so extensive, and still promises so rich a harvest to those inclined to be diligent in the pursuit, that I cannot help inviting

* It is hardly a contradiction of this statement to admit that Christian Mayer, of Mannheim, in 1781, collected all the Double Stars then known (89 in number) into a Catalogue which was published in Bode's *Astronomische Jahrbuch* for 1784. The details given, such as they were, were of the roughest.

† *Phil. Trans.* Vol. LXXII. p. 112. 1772.

‡ *Phil. Trans.* Vol. LXXV. p. 40. 1775.

§ *Memoirs. R.A.S.*, Vol. I. p. 166.

¶ *Phil. Trans.* Vol. XCIII and Vol. XCIV.

** *The Herschels and Modern Astronomy*, p. 55.

every lover of astronomy to join with me in observations that must inevitably lead to new discoveries. I took some pains to find out what Double Stars had been recorded by astronomers; but my situation permitted me not to consult extensive libraries, nor, indeed, was it very material: for, as I intended to view the heavens myself, Nature, that great volume, appeared to me the best catalogue upon this occasion."*

Herschel's Double Star observations failed to assist him in the primary purpose for which he undertook them, which he afterwards described as a desire to ascertain "a scale whereby the extent of the Universe, so far as it is possible for us to penetrate into Space, may be fathomed." But, notwithstanding this primary failure, there was unexpectedly evolved a discovery of a different sort and of the highest interest. This was that many pairs of stars were not only, as they seemed to be, two stars side by side, but were physically connected, so that the smaller revolved round the larger, very much as our ordinary planets revolve round the Sun. Accordingly he was able, after watching certain of them for about twenty years, to inform the Royal Society that he would soon be in a position to communicate "a series of observations proving that many of them have already changed their situation in a progressive course, denoting a periodical revolution round each other." He fulfilled his pledge the following year in a paper presented to the Society on June 9th, 1803. He had taken specially in hand the following six stars:—Castor (α Geminorum), γ Leonis, ϵ Boötis, ζ Herculis, δ Serpentis, and γ Virginis; and he made it clear that the facts which he had observed could only be accounted for by supposing that the six pairs in question were "not merely double in appearance, but must be allowed to be real binary combinations of two stars, intimately held together by the bond of mutual attraction."† He went on to assign periods of revolution for each of the six Stars in question, but, with one exception, it afterwards turned out that this feature in his results failed to find subsequent confirmation, because his observations were not sufficiently prolonged in time; and he assumed the orbits to be more or less circular, whereas all are ellipses of great eccentricity.

In the case of Castor he was able to base his calculations on some observations of the star older by far than his own, dating from 1718, 1719, 1722, by Pound and Bradley, coupled with a similar old record by Bradley and Maskelyne, dating from 1759. These old materials‡ enabled Herschel to suggest a period for Castor of about 342 years, which was, however, eventually found to be altogether insufficient by most computers, though one of the most recent calculations by Doberck puts the period at 346 years. Other computers, however, put it at a period varying from 300 to 900 years, and Burnham says that the matter must remain uncertain for another century.

The ground having been broken by Sir W. Herschel, who had been knighted for his eminent services to science, Double Star work was taken up shortly before his death by F. G. W. Struve, who in 1822 published his first catalogue of Double Stars compiled from various sources, but followed by a catalogue of 795 stars observed by himself at Dorpat, 1813-1820. Several other Struve catalogues followed which will be alluded to further on.

In England the mantle of Herschel fell on his son John, then starting his career. As regards Double Stars he entered for a time into an alliance with Mr. (afterwards Sir James) South who had an observatory in Blackman Street, Southwark.§ Hence it is that in the older records of Double Stars one sees linked together the names of "Herschel and South."

John Herschel, after doing various things, especially after reviewing and cataloguing afresh his father's nebulae and clusters, and discovering and measuring a large number of pairs, thought that the labours of his father and himself could only be properly capped by an efficient and comprehensive survey of the Southern Hemisphere. This led to his expedition to the Cape, where he remained from 1835 to 1838, the grand results of which were given to the world in 1847, at the cost of the then Duke of Northumberland, a liberal patron of the science.¶

Further details respecting this interesting stage in Herschelian work would take me beyond the reasonable limits of this paper. I must, therefore, briefly sum up the modern situation as regards Double Star Astronomy by mentioning in the briefest possible form the names of a few of the principal workers in this field. The following seem to stand out in advance of all others. In England:—W. R. Dawes, W. Doberck, T. E. Espin, W. S. Jacob, G. Knott, T. Lewis, W. H. Maw, G. M. Seabroke and J. M. Wilson. On the Continent:—G. Van Biesbroeck, Baron H. Dembowski, N. C. Dunèr, S. De Glasenapp, J. H. Mädler, J. Perrotin, H. C. F. C. Schjellerup and A. Secchi: and in America, S. W. Burnham, R. G. Aitken, G. C. Comstock, E. Dolittle, J. M. Gillis, A. Hall, G. W. Hough, W. J. Hussey and O. Stone.

It will be seen from the foregoing that a great deal of Double Star work has been done during the last fifty years, but as so many pairs of stars are in motion the necessity for continuing observations of this sort may be considered chronic; and it has been already pointed out elsewhere that very much remains to be done in the Southern hemisphere, which offers a limitless field for the labours of competent Double Star observers.

* *Phil. Trans.* Vol. LXXII. p. 97, 1772. This extract appears in a very garbled form in Miss Clerke's *Herschels*, p. 55.

† *Phil. Trans.* Vol. XCIII. p. 340, 1803.

‡ Discussed by J. Herschel in *Mem. R.A.S.*, Vol. V, p. 23, 1833.

§ South afterwards migrated to Paris, but ended his Astronomical career at Campden Hill, Kensington.

¶ *Results of Astronomical Observations made at the Cape of Good Hope.*

MUCOR RACEMOSUS FRES.

By W. B. GROVE, M.A.

Botanical Laboratory, Municipal Technical School, Birmingham.

On looking at the text-books of Fungi and the Fungus-Floras published in this country, one is struck by the almost total absence of any reference to *Mucor racemosus*, although it is frequently referred to in books on fermentation and brewing. Yet this species is one of the most widely-spread of the Phycomycetes, and must be frequently met with: but it is, no doubt, mistaken for something else, just as *Rhizopus nigricans* is constantly seen mounted on microscopic slides with the label, "*Mucor mucedo* Linn." The fault lies, perhaps, in the absence of any good description of the fungus in an accessible publication—a want which the following notes may possibly supply.

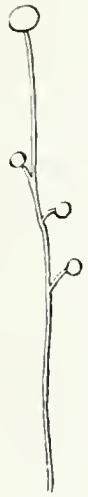


FIGURE 1.
The terminal portion of a branched hypha, × 60.

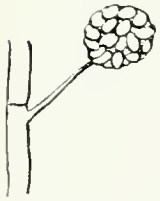


FIGURE 2.
A single lateral sporangium, × 250.

M. racemosus is met with on all kinds of substrata, on starch-paste, potatoes and the like; on rotting vegetation, dried fungi, and herbarium specimens; and also on dung, mixed with *M. mucedo*; on meat and on dead flies lying in water. It is a frequent weed in the cultures of the inexpert bacteriologist in Petri-dishes and similar preparations. On the latter it forms a thin stratum of a pale fawn colour or buffy-brown, seldom more than half-an-inch in depth, often considerably less.

In order to recognise it with certainty it must be premised that the genus *Mucor* can be divided into two distinct sections: first, the division *Mono-mucor*, in which the sporangiophore is, with rare exceptions, always unbranched; and, second, a division in which the sporangiophore is always branched, except when it is very young. The branched forms may be further sub-divided into two sections—*Racemo-mucor*, in which the main hypha remains clearly distinct and the branches distinctly lateral, *i.e.* it branches monopodially—and *Cymo-mucor*, in which the main hypha is frequently thrust to one side and overtopped by the originally lateral branch, *i.e.* it branches sympodially. But these two sections are not always so distinct as might be imagined; some species in fact combine both modes of branching in the same growth. *M. racemosus*, as its name implies, belongs to the sub-division *Racemo-mucor*; *M. mucedo* belongs to *Mono-mucor*, and is much taller, reaching a height of even several inches.

The sporangiophore of *M. racemosus* is erect and colourless, with many short, simple, lateral branches, each of which bears a sporangium smaller than that of the main hypha, and is usually nearly straight or bent slightly downwards (Figure 1). Above the insertion of each branch there is a septum (Figure 2). The sporangia are small, round, varying much in size, (average 40 μ), erect or nodding, yellowish or yellowish-brown, never black, but always semi-transparent so as to allow the spores to be seen through the membrane. The membrane is smooth, but slightly raised in rounded bulging elevations by the outer spores of the contained mass: the most striking difference from *M. mucedo* is seen in the fact that, on the addition of water under the cover-slip, the membrane does not instantly break up and vanish, as in the latter, but merely splits from the pressure and remains otherwise intact (Figure 3). It is yellowish, and finely punctate all over with minute crystals of oxalate of lime, which do not assume the needle-like shape found in *M. mucedo*.

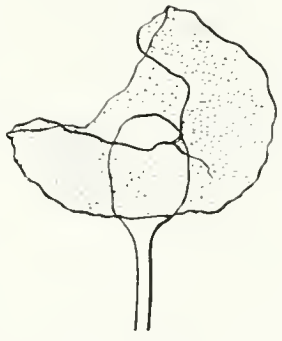


FIGURE 3.
A sporangium emptied of its spores, × 500.

FIGURE 4.
Spores, × 500.

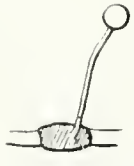


FIGURE 5.
A germinating chlamydospore, × 250.

The columella is roundish or ovate, smooth, and occupies from one-twelfth to about one-fourth of the sporangial cavity.

The spores are roundish or shortly elliptic, 5–8 μ broad, 6–10 μ long, smooth, singly colourless, but slightly yellowish in mass (Figure 4).

The most persistent mark of distinction, however, lies in the fact that all parts of the mycelium are provided with a great abundance of chlamydospores. A chlamydospore is a portion of the protoplasm of the mycelium which separates itself from the rest, surrounds itself with a cell-wall, and becomes a kind of resting-spore, which can in suitable circumstances germinate and produce a mycelium or even a minute sporangiophore directly (Figure 5). They correspond to what are called *aplanospores* in the Algae. The chlamydospores of *M. racemosus* lie in long rows in the hyphae (Figure 6), and even in the sporangiophore; when young they are rounded and highly granular, afterwards they become oval or barrel-shaped, and are easily distinguished by their refringent contents and their thick, smooth, colourless, and many-layered cell-wall.

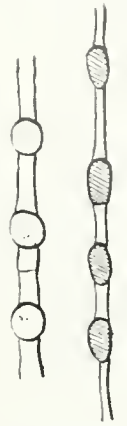


FIGURE 6.
Chlamydospores in the mycelium.
× 250.

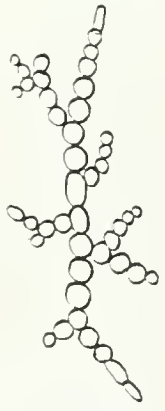


FIGURE 7.
Ferment cells.
× 250.

If a portion of the mycelium of this species is submerged in a sugary fluid it transforms itself into a mass of ferment-cells (Figure 7). These cells are roundish; they produce buds like yeast cells, and like them excite fermentation in the saccharine solution. *M. mucedo* possesses neither chlamydospores nor ferment-cells. *M. mucedo* cannot invert saccharose, in fact *M. racemosus* is almost the only *Mucor* that can do so.

Both these fungi are abundant everywhere, and have been described by authors in the older books on fungi, each under more than a dozen names. The figures and description here given will, however, enable anyone to recognise *M. racemosus* without the slightest difficulty.

THE FACE OF THE SKY FOR SEPTEMBER.

By W. SHACKLETON, F.R.A.S., A.R.C.S.

THE SUN.—On the 1st the Sun rises at 5.13 and sets at 6.47: on the 30th he rises at 5.59 and sets at 5.41. The Sun enters the sign of Libra on the 23rd at 10 p.m., when autumn commences. The equation of time is negligible on the 1st and 2nd, hence these dates are convenient for the adjustment of sun-dials, as only the longitude correction is needed. The positions of the Sun's axis, centre of the disc, and heliographic longitude of the centre are given in the following table:—

Date.	Axis inclined from N. point.	Centre N. of Sun's Axis.	Heliographic Longitude of Centre of Disc.
Sept. 3	21° 38′ E	7° 14′	76° 53′
,, 13	23° 47′ E	7° 13′	304° 50′
,, 23	25° 20′ E	7° 0′	172° 52′
Oct. 3	26° 14′ E	6° 34′	40° 52′
,, 13	26° 25′ E	5° 57′	268° 57′

THE MOON:—

Date.	Phases.	H. M.
Sept. 3	● New Moon	0 6 p.m.
,, 11	☽ First Quarter	8 11 p.m.
,, 19	○ Full Moon	4 52 a.m.
,, 25	☾ Last Quarter	8 54 p.m.
Oct. 3	● New Moon	8 32 a.m.
Sept. 9	Apogee	1 48 p.m.
,, 21	Perigee	10 24 a.m.

OCCULTATIONS.—The following are the particulars of the occultations of the brighter stars visible from Greenwich. The bright star δ Scorpii is occulted just before sunset, but in consequence of the brightness of the star the phenomenon can be observed with telescopes of about four inches aperture:—

Date.	Star's Name.	Magnitude.	Disappearance. Mean Time.	Disappearance. Angle from N. point. E.	Reappearance. Mean Time.	Reappearance. Angle from N. point. E.
			p.m.		p.m.	
Sept. 14	60 Sagittarii	4·8	6.5	68°	7.23	276°
,, 14	A Sagittarii	4·9	7.56	68°	9 15	265°
,, 17	τ^1 Aquarii	5·6	9.30	91°	10.32	207°
			a.m.		a.m.	
,, 17	τ^2 Aquarii	4·4	10.50	55°	12.2	239°
,, 20	α Piscium	5·0	11.58	32°	1.1	258°
					1.m.	
,, 23	30 Tauri	5·2	10.58	127°	11 28	188°
Oct. 7	δ Scorpii	2·3	5.6	111°	6.23	280°

THE PLANETS.—Mercury (September 1st. R.A. $12^h\ 15^m$; Dec. S. 4° 25′. October 1st. R.A. $11^h\ 49^m$; Dec. N. 0° 5′) is an evening star in Virgo during the early part of the month, and sets at 7.10 p.m. on the 3rd. The planet is, however, not well placed for observation.

On the 26th, Mercury is again in inferior conjunction with the Sun.

Venus (September 1st. R.A. $9^h\ 15^m$; Dec. N. 16° 43′. October 1st. R.A. $11^h\ 37^m$; Dec. N. 4° 2′) is a morning star in Leo, very little observable, as she rises in the N.E. on the 1st at 3 a.m., and on the 31st at 4.30 a.m. The apparent diameter is diminishing, being now about 10″, whilst 0·96 of the disc is illuminated, thus the planet appears nearly at the "full" phase when seen in the telescope.

Mars (September 1st. R.A. $11^h\ 13^m$; Dec. N. 6° 6′. October 1st. R.A. $12^h\ 24^m$; Dec. S. 1° 43′) is inobservable, being in conjunction with the Sun on the 27th.

Jupiter (September 1st. R.A. $12^h\ 57^m$; Dec. S. 4° 51′. October 1st. R.A. $13^h\ 20^m$; Dec. S. 7° 14′) is getting more to the West, and at the end of September the planet will be too low down in the West at sunset to be observed. On the 16th the planet sets at 7 p.m.

No satellite phenomenon is observable this month, as the planet is situated in too bright a portion of the sky. The Moon appears near the planet on the evening of the 6th.

Saturn (September 1st. R.A. $2^h\ 20^m$; Dec. N. 11° 11′ October 1st. R.A. $2^h\ 15^m$; Dec. N. 10° 39′) is a conspicuous object in the evening sky looking East, and is situated about 10° South of α and β Arietis. The planet may readily be distinguished from other planets by his leaden hue, and, although a bright object, his brilliancy lacks the sparkling qualities of the other bright planets.

On the 1st September the planet rises at 8.37 p.m., and on the 1st October at 6.46 p.m., remaining visible throughout the night, the planet is thus well placed for observation. The apparent diameter of the ball is 18″, whilst the diameters of the major and minor axes of the outer ring are 45″ and 14″ respectively; thus, as seen in the telescope, the ring appears well open, being inclined to our line of vision at an angle of 18°, the Southern surface being visible.

Saturn is one of the easiest of objects to observe in the telescope, as usually good seeing can be obtained with this planet on poor nights sufficient to spoil all definition on other objects.

In a three-inch telescope the ring should be visible with a magnifying power of about fifty, and the belts on the planet's surface with a power of about eighty.

The Moon appears near the planet on the evening of the 21st, Saturn being one and a half degrees to the South.

Uranus (September 15th. R.A. $19^h\ 33^m$; Dec. S. 22° 16′), though rather low down in the sky, is fairly well placed for observation during the early evening, and is due South shortly after dusk. The planet is situated in Sagittarius in a part of the sky devoid of good reference stars, though the 4th magnitude double star h Sagittarii is about 2° to the South. The planet is at the stationary point on the 1st October, after which his motion is direct or Easterly.

Uranus can just be discerned with the naked eye on clear nights, but the slightest optical aid is sufficient to make the planet clearly visible.

Neptune (September 15th. R.A. $7^h\ 31^m$; Dec. N. 21° 10′) rises about midnight near the middle of the month, and hence is not in a favourable position for observation. The planet is

situated about four degrees South-East of the star δ Geminorum.

METEORS:—

Date.	Radiant. R.A.	Dec.		
Sept. 3-8 ..	353	N. 39°	υ Andromedids.	Very swift.
,, 5-15 ...	62°	N. 37	ϵ Perseids.	Swift, bright streaks.
,, 21-27 ...	87°	N. 43°	β Aurigids.	Very swift.

Minima of Algol occur on the 18th at 12.35 a.m., the 20th at 9.24 p.m., and on the 23rd at 6.13 p.m. The period is $2^d\ 20^h\ 49^m$, from which other minima may be calculated.

TELESCOPIC OBJECTS:—

DOUBLE STARS.—ζ Ursae Majoris XIII.^{h} 20^m, N. 55° 25′, mags. 2, 4; separation, 14″·4.

ζ Aquarii XXII.^{h} 24^m, S. 0° 32′, mags. 4, 4; separation, 2″·9. Both components are yellowish.

β Cygni XIX.^{h} 27^m, N. 27° 46′, mags. 3, 5; separation, 34″ The brighter component is yellow, the other blue; very easy double in small telescopes with a power of 20.

Cluster (M 11) in Aquila or Antinous. R.A. $18^h\ 46^m$; Dec. S. 6° 23′. Very pretty object for three or four-inch telescopes; it is an easily resolvable fan-shaped cluster, with an eighth magnitude star in apex, and an open pair of the same magnitude just outside it.

NOTES.

ASTRONOMY.

By G. F. CHAMBERS, F.R.A.S.

HALLEY'S COMET AS SEEN IN SOUTH AFRICA.—The following account, dated May 16th, 1910, of Halley's Comet as it appeared in Cape Colony, has been sent by Mr. H. R. Butcher:—

Your readers may be interested to hear that Halley's Comet presented a magnificent appearance this morning, viewed from an elevation of over four thousand feet above sea level.

Owing to the fact that the Comet is south of the sun, it can be best seen in the southern hemisphere, and the exceptional clearness of the atmosphere on the Karoo plains of South Africa renders our position a particularly favourable one for observing the Comet during its present return.

The Comet was first visible here to the naked eye on April 13th, and, at its perihelion passage, appeared with a tail of about 2½°. By May 1st the Comet had increased considerably both in size and brightness, and I estimated the length of the tail on that date at 8°.

On May 9th the Comet was a striking object, the nucleus being about equal to a second mag. star, and the tail, 18° in length.

Since May 10th, owing to the swing of the Comet towards the earth, there has been a rapid increase in brilliance and in the apparent length of the tail. Although the head of the Comet is rapidly receding towards the sun, the tail reaches higher and higher each morning into the sky.

This morning the nucleus rose at 5 a.m., two hours before sunrise, and as the planet "Saturn" rose at exactly the same time a few degrees further south, I was able to observe the relative brightness of the two objects. The diameter of the Comet's nucleus was considerably greater than that of the planet, but it shone with a duller light, and lacked the brilliance of Saturn. That the brightness of the two objects was almost exactly equal is proved by the fact that, as the daylight increased and stars faded, the nucleus and the planet became invisible, almost simultaneously, half an hour before sunrise. The tail presented a most striking appearance at 5.10 a.m. It appeared to be perfectly straight, but broadened gradually from the head. It could be distinctly traced for a distance of 55° from the head, reaching to the border of the constellations Aquarius and Pegasus about R.A. 22^h and Decl. 3° North, whilst the position of the nucleus was about R.A. $1^h\ 40^m$, Dec. 15° North. A fourth magnitude star, presumably "η Piscium," could be seen shining through the densest part of the tail without any apparent enfeebling of its brilliancy.

With regard to the supposed connection between Halley's Comet and the Aquarid Meteors, referred to by Mr. W. F. Denning in the April number of "KNOWLEDGE," it is reported that a fine shower of meteors and one remarkably large fireball were observed from Cape Town on the morning of May 7th. The sky was, unfortunately, cloudy here on that date, but I have observed small meteors, coinciding with the Aquarid radiant point, almost every morning since May 1st, but only perhaps half-a-dozen in an hour's time. On several mornings also I observed meteors whose radiant point coincided with the tail of Halley's Comet, so that they appeared to fall out of the tail.

BOTANY.

By PROFESSOR F. CAVERS, D.Sc., F.L.S.

TOXIC SOLUTIONS AND NUCLEAR DIVISION.—Stockberger (*Bot. Gaz.*) has just published an interesting paper, dealing with the influence of poisonous solutions upon the process of nuclear division or mitosis. Some of his results differ markedly from the accounts given by previous workers in this field. For instance, Stockberger claims (1) that the toxic substances (copper sulphate, phenol, strychnin) with which he treated the roots of broad-bean seedlings did not in any case produce direct nuclear division (amitosis); (2) that doubling of the nucleolus is not a preparatory stage of amitosis, as stated by Wasielewski; (3) that no structures were found corresponding to the large fusion-nuclei, with doubled number of chromosomes, said by Nemec to be produced by poisonous solutions acting on bean roots; (4) that the influence of poisons on mitosis is due to the great sensitiveness of the achromatic structures, the spindle fibres being disorganised so that the further progress of division is inhibited. The most important conclusion arrived at by the author, however, is that distilled water is itself a "toxic solution." If this be confirmed, it must be inferred that various abnormalities of cell behaviour which have been attributed to the effect of toxic salts are not necessarily so produced, but may be due to the osmotic action of the solution. It would appear, also, that the practice of growing plants in distilled water—as "controls"—in various physiological experiments is open to serious objection, since such controls are themselves under abnormal conditions, and show the same progressive decline of cell function that occurs in "toxic" solutions, though at a slower rate.

GERMINATION OF RUST SPORES.—As is well known, the spores of the Rust-Fungi (*Uredinaceæ*) germinate in an extremely irregular manner, and, until recently, no satisfactory explanation of their apparently erratic behaviour in cultures has been given. Schaffnit (*Ann. Myc.*, 1909), from various observations and experiments, concludes that the capacity of Rust spores for germination depends largely on the degree of maturity of the spores. The majority of the spores which are shaken off by rain or by wind do not germinate; and spores which have been removed prematurely are incapable of ripening afterwards. Nor can spores ripen on leaves that have

been cut from the host-plant. To obtain quite ripe spores, which will germinate readily, it is necessary to gather them on a warm, dry, still day. This memoir is of importance since it opens up a wide field for further work on the ecology of fungi: our knowledge of the dispersal and germination of fungus spores in nature is very scanty, even as to fungi of economic importance.

THE "SPIKE" OF THE OPHIOGLOSSACEAE.—There has been much discussion concerning the nature of the spike, or sporangiophore, or fertile leaf-segment, of the Adder's-tongue (*Ophioglossum*), the Moonwort (*Botrychium*), and their allies, together forming the group Ophioglossales, which is now regarded as standing quite apart from the true ferns (*Filicales*). The question has now been attacked from the standpoint of vascular anatomy by Chrysler (*Ann. Bot.*, 1910), who has obtained some important results. The "fertile spike" is regarded as representing two fused leaflets—the lowest pair—of a pinnate fern leaf. It must be admitted that this is *clearly* indicated only in certain species of *Botrychium*, but it is difficult to see how the facts brought forward by Chrysler can be interpreted otherwise than as almost conclusive evidence that the "spike" represents two fused pinnae, or (in *Helminthostachys*) a single pinna, and that the "sporangiophore" theory has been disposed of. Chrysler suggests that the Ophioglossaceae have sprung from near the level of the Osmundaceae among the lower ferns.

THE LIFE HISTORY OF LAMINARIA.—A most interesting paper has been published by Drew (*Ann. Bot.*, 1910), a worker at the Plymouth Marine Biological Laboratory, from which it appears that the reproductive organs of the common Sea Tangle (*Laminaria*) are not merely sporangia, producing zoöspores which give rise directly to new plants, but are *gametangia*, producing *gametes* or sexual cells which conjugate in pairs to form zygospores. The zygospore gives rise to a chain or mass of cells, and from this the young *Laminaria* plant grows out directly. The gametes when set free into the water apparently have no cilia, but soon develop these. Drew considers that *Laminaria* shows an alternation of generations, the plant itself being the *x* generation, while the "chain or mass of cells" arising from the zygospore represents the *2x* generation. This inference is, however, rather doubtful; no cytological evidence is given, and the direct outgrowth of the young *Laminaria* plant from the supposed *2x* generation is not suggestive of an intervening reduction division. It is greatly to be hoped that the cytology of *Laminaria* will shortly be worked out in detail.

APOSPORY IN MOSSES.—Some time ago, El. and Em. Marchal showed that when a leafy Moss-plant is induced to grow from a fragment of the stalk of a Moss capsule—that is, when apospory occurs and the sexual generation arises directly from the asexual without the intervention of the spore itself—the Moss-plant thus obtained is always hermaphrodite, or monoecious, even in species which are normally dioecious, e.g. *Mnium hornum*. These authors have followed up their earlier work, and in a recent paper (*Bull. Ac. roy. Belg.*, 1909), they report that these hermaphrodite plants, arising from apospory in dioecious Mosses, are sterile. They also record the following very remarkable observations. (1) The plants arising from apospory in monoecious Mosses show normal sexual characters, and are fertile, producing *4x* sporophytes and *2x* spores, and thus giving rise to a permanent race with bivalent nuclei. (2) The regeneration of the *4x* sporophytes gives rise, in turn, to *4x* gametophytes! This astonishing result follows from the apparent absence of any supplementary reduction or any process of apogamy, by which the doubling of the chromosomes can be avoided. (3) The comparison of homologous organs in the *x*, *2x*, and *4x* gametophytes and in the *2x* and *4x* sporophytes shows the existence of a direct relation between chromosome-number on one hand and the size of nucleus and cell on the other. This increase in size of the cells results in augmented size of the sexual organs themselves. (4) The aposporous origin of the Moss-fruit actually occurs in nature in various species of Mosses, as a result of wounds and other injurious influences, the bivalent races thus produced being readily distinguished from the corresponding normal types by the increased sizes of the sexual organs.

RECENT RESEARCHES ON TRANSPIRATION.—The great importance of transpiration—the giving-off of water vapour by the aërial green organs, especially the foliage-leaves, of plants—has led to a vast amount of research in recent years, doubtless because of the increased attention that is being paid to ecology, and because water forms the most important factor in the environment of plants.

(1) Thoday and Sykes (*Ann. Bot.*, 1909) describe some experiments on what they term "the transpiration current in submerged water-plants." That movement of water occurs in the stems of submerged aquatics has long been known, but, though these new observations certainly give clear evidence of such movement, it is surely misleading and erroneous to apply the term "transpiration current" in the case of these plants, in which there cannot be any question of a true transpiration process.

(2) Bergen (*Bot. Gaz.*, 1909) in an interesting paper on "the modifiability of transpiration in young seedlings," concludes from his experiments that plants grown in a highly humid atmosphere acquire a much greater than normal capacity for transpiration in a moderately dry atmosphere; that different genera of the same family vary greatly in their capacity to acquire by such culture a tendency to extremely rapid transpiration; that the transpiration rate becomes notably greater as the leaf becomes fully developed; and that transpiration is not necessarily greater when the relative humidity of the air is very low than when it has a medium value.

(3) Sampson and Allen (*Minnesota Bot. Studies*, 1909) set out to determine the influence of physical factors on transpiration. They find that plants developed in sunlight lose from two to four times as much water as plants of the same species developed in shade; that increase in altitude produces increased transpiration, the effect being due *solely* to the diminished barometric pressure; that transpiration is directly related to internal structure in amphibious as compared with land plants, the chief factor being the presence in the former of abundant air-chambers in the stem; that acid solutions accelerate and alkaline solutions retard transpiration, and weak solutions often produce as marked effects as strong ones.

(4) Puglisi (*Annali di Bot.*, 1909) devotes a long and detailed paper to the transpiration of Dicotyledonous evergreens in winter, spring and summer. He investigated the rate of the transpiration current in detached shoots by means of the potometer—his results showing certain discrepancies due to unwary use of this apparatus, by the way; and he also compared, by means of Garreau's well-known method, the transpiration from the upper and lower sides of the leaves experimented with. Puglisi's chief conclusions are: that the volatile oils present in evergreens are highly efficient in preventing excessive transpiration; that the safeguards against injurious transpiration do not prevent rapid transpiration when there is an adequate water supply from the roots; that the relative transpiration rate of the upper and lower leaf surfaces varies decidedly at different seasons; and that for evergreens transpiration shows a minimum in winter, a maximum at the beginning of spring, and a lowering to, or nearly to, the winter rate during the heat and drought of summer.

(5) Reed (*Bot. Gaz.*, Feb., 1910) has made a careful study of the effects of certain chemical agents, chiefly salts, upon the transpiration of wheat seedlings, in relation to the effect of the same compounds upon the growth of the plants. He finds that transpiration is increased by lime, by sodium phosphate, and by tannic acid; it is diminished by potassium salts, and usually by sodium nitrate; while the effects of organic acids are variable. These effects appear in all cases to be a specific action of the ions constituting any given agent. For instance, potassium always inhibits, while calcium always accelerates transpiration, whether combined to form chloride, nitrate, or sulphate. Reed notes

that practically all previous writers agree in attributing to acids an accelerating influence on transpiration, but he points out that in his experiments the acids employed were added in smaller amounts than those used by Sachs and others; while his experiments were extended for twelve to fifteen days instead of only two to four days.

(6) Wiegand's recent paper (*Bot. Gaz.*, June, 1910) on the relation of hairy and cutinised coverings to transpiration is of special interest from its direct bearing on Ecology, as well as from the beauty and ingenuity of the author's methods. He first made a series of experiments, in which blotting paper was used as the evaporating surface, his plan being to expose saturated square pieces, all of the same size, to the air, each having been previously weighed and covered with the material used to stimulate a protective covering—cotton to represent hairs, beeswax to represent cuticle. The evaporation experiments showed that porous hairy coverings must be very thick to produce any appreciable effect in retarding evaporation if the surrounding air is still, but become very efficient even in thin layers when the air is in motion. On the other hand, a waxy covering is effective at all times, though, of course, somewhat more so in wind. In sunshine, also, the hairy covering shows a greater increase in efficiency than does wax. It would appear that a hairy covering is employed to retard transpiration by plants which grow in situations where a normal water supply is available, and where transpiration must be reduced in excessively dry times, but not interfered with when the surrounding air is damp and transpiration therefore difficult. Cutin, on the other hand, is probably employed where there is considerable danger of too great desiccation at all times.

CHEMISTRY.

By C. AINSWORTH MITCHELL, B.A. (Oxon)., F.I.C.

THE CHEMISTRY OF SOME TANNING AGENTS.—In addition to oak bark, chestnut bark and sumach, there are numerous vegetable products used as the source of tannin in the manufacture of leather, some of which are but little known outside the circle of those engaged in the industry. The most important of these tanning agents are divi-divi, myrobalans and valonia, each of which has its own effect upon the leather.

DIVI-DIVI, which was first introduced into Europe towards the end of the eighteenth century, is the commercial name of the dried pods of a large shrub, *Cæsalpinia coriaria*, which grows in swampy districts in South America.

FIGURE 1. Divi-Divi Pods.

In the dried condition the pods are curious looking objects, which, when uncurled, are an inch and a half to three inches in length. They are dark brown in colour and have a very astringent taste, due to the tannin, which is chiefly concentrated in the rind beneath the epidermis.

As a rule they yield from thirty to fifty-two per cent. of tannin, and a commercial sample examined by the writer contained thirty-six per cent.

The tannin closely resembles the gallotannic acid from galls, both in composition and reactions, but differs from it in yielding a deposit of ellagic acid when dissolved in water and heated in a sealed tube. Hence Löwe (*Zeit. anal. Chem.*, *11*, 35), described divi-divi tannin as *ellagitannic acid*, and ascribed to it the formula, C_{14} H_{10} O_{10}, which may be represented as gallotannic acid, C_{14} H_{10} O_9, plus one atom of oxygen.

MYROBALANS are the dried fruit derived from different species of *Terminalia*, indigenous to India and the East Indies.

The so-called "black" or "chebulic" myrobalans, which are also widely employed as a black dye, are the product of the Indian species, *T. chebula*.

A commercial sample examined by the writer contained thirty-nine per cent. of tannin, which behaved towards reagents in the same way as divi-divi tannin. In fact Löwe (*loc. cit*) concluded that the two tannins were identical—both consisting of ellagitannic acid.

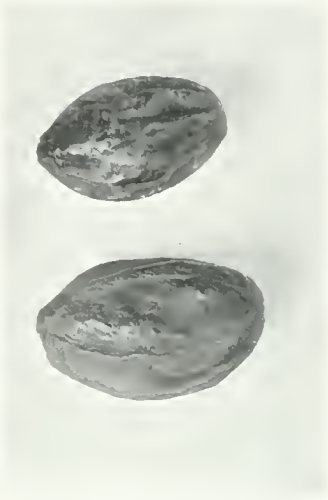


FIGURE 2. Myrobalans.

VALONIA is the name given in commerce to the dried acorn cups of certain kinds of oak growing in Greece and Asia Minor. The best kinds are obtained from *Quercus ægilops* and *Q. macrolepis*, and are gathered in spring before the fruit matures. The valonia beaten from the trees in the autumn contains less tannin, and consequently fetches a lower price in the market. The freshly-gathered fruit is partially dried on the spot, and sent by mules to Smyrna, where it is allowed to stand for a partial fermentation to take place, so as to detach the acorns from the cups, the latter being then dried and exported. Excessive fermentation, such as may take place if the acorns are exposed to rain after being gathered, causes the valonia to turn black and to be deficient in tannin.

FIGURE 3. Valonia.

The ordinary commercial product is in the form of semi-circular, prickly cups, which may contain anything between twenty and forty-five per cent. of tannin, according to the species of oak whence they were derived and the season of gathering.

The tannin appears to be identical with that contained in divi-divi and myrobalans, but in the impure state differs more than the latter from gallotannic acid. Thus, on the addition of an iron salt it forms a compound with a much bluer tinge. It

is possible, however, that this may be due to the formation of gallic acid by fermentation from the *ellagitannic* acid and the results of some experiments made by the writer support this view.

THE INK-PLANT.—By a slip, the illustration of *Coriaria myrtifolia* in the last issue, was described as the "ink-plant," whereas the name should have been restricted to the other member of the family, *C. thymifolia*.

ACTION OF SULPHURETTED HYDROGEN ON WHITE LEAD.—It is a matter of common knowledge that white lead paints exposed to the air of towns darken, owing to the action of the sulphuretted hydrogen, derived mainly from the smoke from coal. Experiments recently made by Mr. J. Sacher (*Chem. Zeit.*, 1910, *34*, 647) show that only the lead atoms attached to the hydroxyl group in the white lead re-act with the gas to form lead sulphide. Thus, if the absence of lead hydroxide or of basic lead carbonate from white lead could be ensured, the pigment should not become discoloured. In an experiment in which a current of sulphuretted hydrogen was passed through absolute alcohol in which white lead was suspended, only two per cent. of sulphur was absorbed, the product having a composition agreeing with the formula $4PbCO_3$, PbS, $Pb(OH)_2$. On the other hand, when the white lead was suspended in hot water and the gas passed through the liquid, the product contained five per cent. of sulphur, owing to the formation of basic lead carbonate, $PbCO_3$, $Pb(OH)_2$, by the action of the water on the normal carbonate, and the interaction of the resulting (OH) group with the sulphuretted hydrogen. It was found that the brown lead sulphide formed in these experiments was not bleached after being exposed to sunlight for three years in a sealed glass tube.

ECONOMIC BIOLOGY.

By Walter E. Collinge, M.Sc., F.L.S., F.E.S.

WEEDS.—The subject of weeds and their control is one that must ere long receive more than passing attention. The manner in which Urban and District Councils, Railway Companies, and Land Owners permit weeds to grow and flourish upon their land is one that calls for very drastic steps, for the losses to farmers and horticulturalists through the neglect of such land must be enormous. Unfortunately, in this country, we have no law which makes it incumbent upon the people mentioned above to destroy the weeds, which flourish upon their land and spread their seeds on to neighbouring cultivated land. Hitherto the farmer has looked on in silence, but there are signs that he is beginning to realize that a plague of weeds is as bad as or worse than a disease amongst his live-stock, and very soon he will demand that those who have the control of land adjoining his shall at least keep it free from plants which rob him of part of his crop, that encourage injurious insects, fungi, etc.

DISADVANTAGES OF WEEDS.—In a recent paper, Dr. W. J. Beal (*Mich. Agric. Exp. Station, Bull. 260*) points out in a very pertinent manner the disadvantages of weeds. That they rob cultivated plants of nutriment and injure crops by crowding and shading is generally appreciated, but they also retard the work of harvesting by increasing the draft, and extra wear of machinery. The increased labour of threshing and the cleaning of seed are also points to be considered. Very few weeds are of any value as food for domestic animals, whilst many are injurious, especially those having barbed awns and those that contain strong acid or other objectionable contents which injure the quality of dairy products and the taste of meat. Some, of course, are distinctly poisonous.

Amongst the few advantages, he mentions that they induce more frequent and more thorough cultivation, which benefits crops, and in occupying the soil after a crop has been removed they prevent the loss of fertility by shading the ground, whilst if ploughed under they add some humus and fertility to the soil.

THRIPS.—The increased interest that has been taken in economic biology of recent years has indirectly been the means of directing attention to some of the minute and less known animals and plants, such, for instance, as the Protozoa, Ticks, Springtails and Thrips. The latter insects we are pleased to note are receiving very thorough and careful treatment at the hands of Mr. R. S. Bagnall, a number of whose recent writings are before us. As is frequently the case with neglected orders or families, the points of interest zoologically and economically are almost endless, and the careful study Mr. Bagnall is making of these insects is bringing to light many of far-reaching importance.

ANAPLASMOSIS OF CATTLE.—The disease of cattle known as anaplasmosis is due to the presence of a somewhat remarkable protozoon (*Anaplasma marginale*), which, so far, has no simile in protozoology, so Dr. Theiler informs us in a most valuable article in the last issue of the *Transvaal Agricultural Journal*. This protozoon consists of only a chromatic substance—a nucleus, without the surrounding protoplasmic body. Occasionally around the parasite there can be seen a paler zone, the globule apparently not staining here as well as the rest. Reproduction takes place by fission. They multiply rapidly and in susceptible animals invade and destroy a large percentage of the red blood corpuscles. As a rule the parasites are situated on the periphery of the corpuscles, but they are also found in the centre of the corpuscles. The disease is found all over Africa and has also been described from North America and the Transcaucasus. No experiments have yet been made with the modern drug "Trypanblau," which seems to have a beneficial influence in allied diseases.

GEOLOGY.

By G. W. Tyrrell, A.R.C.S., F.G.S.

CARBONIFEROUS GLACIAL BOULDER-BED IN WEST AUSTRALIA.—An addition to the already long list of Palæozoic boulder-beds has been made in West Australia by the Government Geologist, A. Gibb Maitland. It occurs among the Basal Beds of the Carboniferous in the Gascoyne River Valley. The best sections, however, are exposed in the Arthur River, where the boulder-bed, whose thickness is not mentioned, rests on sandy and flaggy limestones, and is overlain by a limestone conglomerate. The matrix is a very calcareous clay, which is crowded with beautifully striated boulders. The boulder-bed is seldom seen in place, although its presence is always indicated by the heterogeneous collection of boulders resulting from its weathering. It forms a valuable stratigraphical horizon, which may be followed across country for scores of miles. The occurrence is described in the recently issued Bulletin, No. 33, of the Geological Survey of West Australia.

NEPHELINE SYENITES OF EASTERN ONTARIO.—The enormous areas of nepheline syenite in Ontario, said to be the largest development of this rock in the world, have been recently described by Messrs. Adams and Barlow in the Transactions of the Royal Society of Canada. These rocks have attained a considerable economic importance on account of the abundant development of corundum. The magma was apparently supersaturated with alumina, which, crystallizing out as corundum, has given rise to several new rock types. The rock is further remarkable for its extraordinarily perfect gneissic structure, which, combined with a banding due to original heterogeneity of the magma, causes an extreme and rapid variation of composition and a great diversity of types within a short distance. The nepheline syenites form part of the Precambrian complex of the Canadian Shield, and occur along the borders of batholiths of Laurentian gneiss, where these cut the crystalline limestones of the Grenville series.

"THE IRON ORE RESOURCES OF THE WORLD."—This magnificent work in two volumes, with an atlas, has just been issued under the auspices of the Eleventh International

Geological Congress at Stockholm. It forms, perhaps, the most finely-conceived and well-executed work on economic geology yet published. The publication takes the form of separate reports on various countries or districts drawn up by experts familiar with their iron ore deposits. A valuable summary of the reports is contributed by Prof. Sjögren. For some areas, such as Europe and the United States, it has been found possible to estimate the iron ore resources with some degree of exactitude; but in the case of little-known or undeveloped areas, such as most of Canada, the information obtainable is very meagre. For practically unknown areas, such as the interiors of Asia and Africa, no reports were possible. The deposits concerning which statistics were obtained were classified into three groups,—(*a*) those in which a reliable estimate, based on actual investigation, has been made; (*b*) those in which only an approximate estimate could be made; (*c*) those in which a numerical estimate was not possible at all. The statistics indicate an actual reserve amounting to ten thousand million tons, with a potential reserve of at least fifty thousand million. The first figure represents the totals of group (*a*), the second of group (*b*). The reserves in group (*c*) are vaguely described as "enormous." As the production of pig iron is at present estimated at sixty million tons per annum, the actual known reserves of iron, quite apart from the potential reserves, would suffice the world for nearly two hundred years on the supposition that the production remained stationary. If, however, the production of pig iron increases at the same rate as at present, that is to say doubling every twenty years, the known supply will only last sixty years. There is, however, no prospect of a famine, for with lapse of time, the deposits now classed as potential will become available, and the advance in technical methods will render possible the utilization of lower-grade ores.

METEOROLOGY.

By JOHN A. CURTIS, F.R.MET.SOC.

THE WEATHER.—The week ending with July 30th opened with cool and unsettled weather, which improved in England towards the close of the week, but which remained changeable in Scotland and Ireland. Thunderstorms were experienced on the 25th and 28th.

The highest temperature recorded in the British Isles during the week was 75°·7 at the Royal Observatory, Greenwich on the 28th. Readings of 74° were reported on the same day at several places in the Midlands and Eastern Counties. The maximum observed in Jersey was, however, only 70°, while at Glencarron, five hundred miles further North, 71° was registered on the same date (28th). The lowest readings ranged from 31° at Balmoral to 54° at Portland Bill, but readings as low as 46° or less were reported in all Districts except the English Channel, where the minimum was 52° on the 27th.

The Rainfall was in excess in the North and West, but was slightly in defect in the Midlands and South-Eastern Counties. The excesses were considerable, and at several stations more than twice the average amount was recorded. At Markree Castle, Sligo, the total amount was nearly four times the average for the week.

Sunshine was deficient in all Districts, the only stations reporting excesses being *Birr* Castle, (0·5 hour excess) and Markree Castle, (3·0 hours excess). Jersey had the largest amount of Sunshine, 51·6 hours (48%), the only other stations reporting more than forty hours being Guernsey (49·8 hours), Ramsgate (44·8 hours), Scilly (42·5 hours) and Margate (40·5 hours).

The temperature on the grass fell to 28° at Balmoral, and to 30° at Sheffield. At no other station was frost recorded, but ground temperatures of 40° or below were experienced in many places.

The highest sea-water temperatures reported were 63° at Margate and Eastbourne, and 62° at Seaford and Newquay.

During the week ended August 6th, the weather was again cool and changeable. Thunderstorms were reported on the 3rd, 4th, and 5th, with, in many cases, heavy falls of rain and hail. Extraordinarily high temperatures were reported from the Shetlands during this period. In the forty years from 1871 the highest temperature reported from *Sumburgh* Head was 70° in June, 1891, in July, 1897, and in August, 1906, but on August 6th of the current year 82° was registered, or 12° higher than the previous maximum. This great warmth was, however, only local, for at Baltasound, about sixty miles to the northward, the maximum was only 77°, while at *Deerness*, Orkney, less than one hundred miles to the S.W., it was only 58°. At Plymouth on the same day the temperature did not exceed 64°, which was the maximum for the week at that station.

At no other station in the British Isles besides *Sumburgh* Head was a temperature of 80° or upwards reported during the week, and in two Districts, England, S.W., and England, N.W., the maximum was only 69°. In the Channel Islands the highest reported was 70°.—at Guernsey on the 3rd. Balmoral reported the lowest temperature—37°, on the 4th, the next lowest being 42° at Cirencester, Newton Rigg, and Markree Castle. At Guernsey the minimum was 55°.

The Rainfall was variable, being deficient in many parts, but in excess in many other parts. In England, N.E., there was but little more than one-fourth the average fall, while in the Channel Islands there was half as much again as usual.

The distribution of Bright Sunshine was also very variable, being, as a rule, in excess where the rainfall was in defect, and *vice versa*. The sunniest stations were Lowestoft with 71·6 hours (66%), and Yarmouth with 69·3 hours (64%) while in the North and West of Scotland, Glasgow had but 17·0 hours (15%), and Fort Augustus only 16·5 hours (15%). London (Westminster) reported 43·7 hours or 41%.

The lowest temperatures on the grass were 32° at Sheffield and 34° at Balmoral. The temperature of the sea at Margate varied between 62° and 64°, while at Wick and Berwick the extremes were 10° lower, namely, 52° and 54°.

During the week ended August 13th, the temperature was above the average, except in England, S.E., where it was slightly (0·5) below, and in the English Channel, where it was normal. The maxima were uniform, the District extremes only varying from 74° to 77°. The highest shade maximum recorded was 77°·4 at Greenwich on the 12th, and the lowest shade minima were 40° at Balmoral, West Linton and Newton Rigg on the 9th or 10th. In Guernsey the range of temperature was from 74° to 55°. The North and West of Scotland were, relatively, the warmest parts of the British Isles, the excesses above the average in those districts being 3°·5. The rainfall was in defect all over the country, and as a rule was less than half the average. At only two of the recording stations were amounts exceeding an inch for the week reported, namely, Castlebay (1·08 inches), and Malin Head (1·02 inches). There were, however, some local falls of exceptional amount, and on the evening of the 11th parts of West London were for a time in flood.

Although the rainfall was in defect, the Bright Sunshine was, in most cases, in defect also, the exceptions being Scotland, N. and E., and Ireland, N. In England, E., the weather was very dull, and at Cambridge only 22·8 hours was reported as against an average duration of 44·4 hours. Torquay reported the largest amount for the week, 49·5 hours (48%) and Cromer the least amount, 13·7 hours (13%).

During the three weeks, July 24th to August 13th, the records show that at the Royal Observatory at Greenwich the highest shade temperatnre was 77°·4 on August 12th, and that the lowest was 47°·4 on July 24th. Rain fell on 14 days, but was only heavy on August 4th, when 0·63-inch was measured. The total amount collected during the period was 1·38-inches. Only one day, August 13th, was sunless, but, on the other hand, only three days had amounts exceeding eight hours, namely August 1st (10·4 hours), 3rd (12·5 hours) and 10th (11·3 hours).

The highest temperature in Sun's rays was 141°·1 on Bank Holiday, August 1st, and the lowest temperature on the grass was 40°·7 on August 3rd.

FLOODS IN JAPAN.—Telegraphic communications report disastrous floods in Japan. In Tokio alone thirty thousand houses are said to be submerged, over eleven hundred persons are reported drowned or missing, and two hundred thousand persons are said to be in receipt of public relief in consequence.

METEOROLOGICAL OFFICE.—Announcement is made that the Meteorological Office is to be removed in the coming autumn to new premises at South Kensington. The Office is at present housed at No. 63, Victoria Street, Westminster, in a building designed for residential flats. The accommodation available there has long been felt to be inadequate, and a new building has now been erected by H.M. Office of Works in the Exhibition Road, which will accommodate the Meteorological Office in its upper part, while the basement and ground floor will be utilized for a Branch Post Office. In connection with this removal some new appointments have been made. Mr. R. G. K. Lempfert, M.A., at present Superintendent of Statistics, becomes the Superintendent of the Forecast Division, and will be succeeded in the Statistical Department by Mr. E. Gold, M.A., Schuster Reader in Dynamical Meteorology in the University of Cambridge. Mr. R. Corlen, M.A., has been re-appointed as Special Assistant to the Director, with additional duties as Secretary and Clerk of Publications.

MICROSCOPY.

By A. W. SHEPPARD, F.R.M.S.,

with the assistance of the following microscopists:—

ARTHUR C. BANFIELD. ARTHUR EARLAND.
JAMES BURTON. RICHARD T. LEWIS, F.R.M.S.
THE REV. E. W. BOWELL, M.A. CHAS. F. ROUSSELET, F.R.M.S.
CHARLES H. CAREYS. D. J. SCOURFIELD, F.Z.S., F.R.M.S.
C. D. SOAR, F.R.M.S.

ON MOUNTING IN HARDENED GLYCERINE JELLY.—Mr. William N. Allen, of the Dublin Microscopical Club, has brought under our notice a method of mounting in glycerine jelly which may prove of some interest to our readers. It is specially applicable to small delicate plants such as Hepaticae, Jungermanniceae, and so on, where it is necessary to preserve the specimen entire for purposes of future examination. The method consists in hardening the glycerine jelly by the use of potassium bichromate. The selected specimen is mounted in the glycerine jelly in the usual way, allowing sufficient jelly to surround the cover-glass to the extent of about one-eighth of an inch. When set a ten per cent. solution of potassium bichromate is painted over the jelly and allowed to soak in for about ten to fifteen minutes, the superfluous solution is then removed by means of filter paper, and the slide put away in a dark place until the solution has permeated the jelly, which will be complete in about twelve hours. The slide is now exposed to daylight until the yellow colour has disappeared. After a week or so the excess of jelly may be removed; the slide does not require either cementing or ringing. The specimen of *Plagiochila asplenioides* mounted by this method some nine months ago, is excellent, and still retains its delicate colour. How far this method can be extended to the preservation of the green freshwater algae, is a subject for experiment which some of our readers may wish to try; most workers in that subject having found the usual methods of preserving green algae to be more or less unsatisfactory.

LEAF-SCALES IN THE GENUS ELAEAGNUS.—The majority of amateur microscopists are quite familiar with the scarious, discoidal leaf-scales found on the under surface of the leaves of *Hippophaë rhamnoides* or *Sea-buckthorn* the solitary British representative of the order Elaeagnaceae. These scales are exhibited in a much more perfect manner on the under surface of the leaves of plants belonging to the allied genus *Elaeagnus*, the best known species of which is the Oleaster, or Wild Olive of our shrubberies. In the case of *Elaeagnus pungens*, the Natsu-Gumi, of China and Japan, the harsh rigid leaves are thickly covered on the under surface with brownish scales, producing a feeling of roughness when the leaves are drawn between the fingers. The scales have the fringed margin turned slightly upwards, presenting a concave surface to the observer. In other species, such as *E. umbellata*, the Masiro-Gumi, of Japan, the under surface of the leaf is of a silvery metallic appearance. The latter is very characteristic of the Silver Berry, *E. argenta*. In these species the fringed margins of the scales are closely adpressed to the surface of the leaf, the silvery appearance being due to the enclosed air. These very characteristic leaf-scales are almost entirely absent from the leaves of *E. orientalis*, where the surface is closely covered with many-rayed stellate hairs, the few scales present being restricted to the mid-rib of the leaf.

THE CULTURE OF FLAGELLATES ON SOLID MEDIA.—The June number of the *Quarterly Journal of Microscopical Science* (vol. lv, part ii) contains a paper by Mr. C. M. Wenyon, Protozoologist to the London School of Tropical Medicine, on "Some observations on a Flagellate of the genus *Cercomonas*." In this paper the author gives details of the culture of such organisms as Flagellates on solid agar media. This may be carried out in an ordinary Petri dish, where the progress of the culture may be watched under a low power of the microscope. A very useful method for this purpose, and one which will allow of observation being made with high powers, is the following:—A long cover-glass (one and a half inches) is taken and carefully cleaned. On a clean slide ridges of Czokor's wax are so arranged about an eighth of an inch high that the cover-glass will form the lid of a box. Some of the medium is melted by placing the test tube containing it in boiling water, and a drop of this is allowed to fall on the previously warmed cover-glass, and, by carefully tilting it, the fluid medium is made to form a thin film; this is now allowed to solidify. The surface of the medium is inoculated with a small quantity of material from a previous culture, and the cover-glass carefully inverted on the wax ridges. By means of a hot wire and more wax the whole is now completely sealed up. It is most essential that this is thoroughly done, as, if an opening be left, it will be found that the medium will quickly dry and the culture cease. In this way it is easy to follow the multiplication of the Flagellates with a one-six inch objective, and if the film of the medium has been made sufficiently thin an oil immersion lens may be employed. Berliner, who first used this method of culture for Flagellates, found that they multiply rapidly, and in every case their chief nourishment seems to be the numerous bacteria that grow on the medium at the same time.

THE MICROLOGIST.—We have received from Messrs. Flatters, Milborne & McKechnie, Ltd., the first number of the *Micrologist*, which will be published quarterly. The main objects for which this Journal is established are to instruct learners in microscopical manipulation, and in the methods of preparing objects for examination, or, in other words, to teach the methods of preserving the results of research. The Journal will, in extent, cover practically all branches of Biology, and will gradually work upwards from the simple to the more complex methods, and will afterwards take special types not generally dealt with in the ordinary text book. It will be illustrated both with figures in the text and with actual photo-micrographs. The present number deals very fully with the methods to be employed in dry mounting, and is illustrated with five photo-micrographs of objects mounted by that method, that representing the dried seeds of *Spergularia marina* being specially worthy of mention.

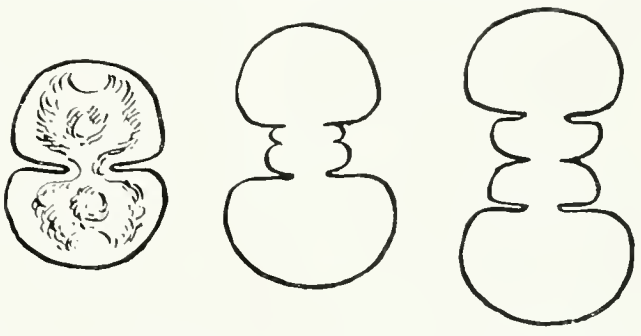
THE EVOLUTION OF THE GREEN ALGAE.—In the current number of *Science Progress*, Dr. Fritsch completes his study of the Phylogeny and inter-relationships of the Green Algae. It is the purpose of the article to indicate briefly the diverse lines of evolution that can at present be traced among the Green Algae, and to consider their connection with the Flagellates. Modern research has shown that we must distinguish two clearly-marked classes amongst the Green Algae—the Isokontae and the Heterokontae, and these are both linked by intermediate forms with the group Flagellatae which shows a curious mixture of plant and animal characteristics, and are probably the survivals of the simplest group of organisms from which both kingdoms have

arisen. The life-history of the unicellular form *Chlamydomonas* is described in detail, and its resemblance in many respects to the Polyblepharidaceae, which latter show a very marked mixture of Chlamydomonad and Flagellate characteristics. The connecting link with the Flagellates is, according to the author, by way of the Cryptomonadineae which, in the possession of chlorophyll and in forming starch show significant analogies to the green algae. From the unicellular the next step is to such forms, as *Gonium* which may be described as a colonial *Chlamydomonas*, and the marvel of perfection in this respect, the genus *Volvox*. This series affords a very striking example of recapitulation in the invariable recurrence of the flat *gonium*-form in the early phases of development. In the second section the author deals with the higher filamentous types, such as the Siphonales and the Ulotrichales, both of which are probably developed from a Protococcaceous stock.

A NEW TRYPANOSOME.—In *Nature*, for August 4th, Professor E. A. Minchin describes a new Trypanosome parasitic in human blood. Its discoverer, C. Chagas, first observed it in the digestive tract of a bug infesting the huts of the poorer people in Brazil, and afterwards found numerous trypanosomes in the blood of the human beings dwelling in this area. The life-cycle of *Schizotrypanum cruzi* shows some peculiarities not yet known in any other species. These differences being that there is an intracorpuscular phase in its development, and that the new species exhibits schizogony or multiple fission; this latter phase takes place in the lungs of experimentally inoculated animals, and it is during this stage the disease proves fatal.

THE QUEKETT MICROSCOPICAL CLUB.—At the gossip meeting, on August 9th, Mr. Hilton was showing specimens of the Mycetozoa. An example of *Lamproderma arcyrionema* was particularly beautiful: under incident light and a low power not only was the elegant form well displayed, but the outer coat of the sporangium shone with a metallic lustre resembling gold, finely contrasting with the dead black capillitium within, which appeared where portions of the covering had become detached. Mr. Draper exhibited, under a binocular, the singular and not very common rotifer *Pedalion mirum*. Its extraordinary appearance makes it a striking object, especially when viewed under a binocular. Another member exhibited several slides showing the non-sexual reproduction of mosses by means of small bulbs formed on the stems and rhizoids, and "gemmae" either in cups on the ends of the stems as in *Tetraphis*, or seated directly on the stem as in *Aulacomnion*. The brilliant red colour of the bulbs in comparison with the green stems made a beautiful object, especially when shown with the dark blue background obtained by a Rheinberg colour disc placed under the sub-stage condenser. This method of illumination is looked at askance by some serious microscopists as being merely a concession to the desire for prettiness; but in its defence it may be urged that, while giving a beautiful effect, when, as in the present instance, the natural colours of the object are brought out sharply by the darker background, at the same time, details not otherwise easily seen, are at once made noticeable; also, that the deeper colour of the background is less trying to the eyes than the glare, a certain amount of which is inevitable with the ordinary method of illuminating transparent objects. J.B.

DESMIDS AT THE EXHIBITION.—On a recent visit to the Japan and British Exhibition, Shepherd's Bush, it was noticed that on the surface of the water of the canals and lagoon there was a very considerable amount of some green floating matter. Various species of *Oscillatoria* and other Myxophyceae are common in such situations, but examination proved that in this case the substance was mainly composed of a small Desmid. The cells were held together in larger or smaller masses by a clear colourless jelly; often this was about ¼-in. thick, the under side having a small amount of soil and dirt adhering to it. It had evidently been formed on the slight quantity of mud and sediment at the bottom of the water; then, owing to sunlight and warmth, the plants had given off gas which entangled in the jelly-like substance, had floated the whole to the surface. The Desmids belong to the large genus *Cosmarium* and were probably *C. nitidulum*, though the identification is made with some hesitation, a detailed account of the genus—a somewhat uncertain one—not being accessible at the moment. Each plant consists of a flattened cell of approximately circular outline, deeply notched, and almost divided by a sinus in the middle into two nearly semi-circular halves. There is a chloroplast, usually fan-shaped, that of the two halves being connected by a narrow band, the colour a bright pale green, and a very evident round body (pyrenoid) in each half. The surface of the cell is smooth, length about 27μ, breadth about 22μ. After being kept in a small aquarium in a window for a few days, many of the Desmids are beginning to propagate. The chlorophyll in each semicell contracts a little and leaves the narrow connecting portion. The two segments of the plant separate slightly, and each puts out a small



A B C

projection of clear protoplasm. This gradually increases in size, so further separating the two parts of the parent, and, at the same time, assuming the shape of the mature semicell. When the new parts are about half grown some of the chlorophyll passes into them from the parent, and they gradually attain full size and development. When this stage is reached there are two complete but connected plants, each consisting of an original and new semicell, and finally, the two entirely separate. The figure above shows the mature plant A, the commencement of division B, and a more advanced stage of the process C. Besides the Desmids there were several other small algae usually found in companionship with them. *Scenedesmus obliquus* and *S. quadricauda*, and varieties being noticed. J. B.

PHOTOGRAPHY—PURE AND APPLIED.

By CHAPMAN JONES, F.C.S., F.I.C.

THE BRUSSELS CONGRESS.—The fifth International Congress of Photography held its meetings during the first week in August. Judging by the number of communications received and the various nationalities represented, it was the most successful congress of the series. The subjects dealt with were so various that it is impossible in a short note to give any general idea of the proceedings, but I shall hope to give, from time to time, references to some of the communications that are likely to prove of interest. The following note refers to papers read at the Congress.

INSTANTANEOUS AUTOCHROMES.—M. C. Simmen has succeeded in increasing the sensitiveness of autochrome plates by eight times, so that useful exposures may be made with large aperture lenses in from the tenth to the fiftieth of a second in a good light out of doors. This is effected by bathing the plates in a solution that renders them much more sensitive to red—a bath containing pinaverdol or pinacyanol. Plates so treated are a great deal more sensitive to the red up to a wave length of 7.000, and show maxima at 5.350, 5.750, and 6.300. It is therefore necessary to use a different compensating screen from that usually employed, but the author finds that by following his instructions the colour sensitiveness of the prepared plates is so well balanced that it is only necessary to stop the ultra-

violet, and this he does with an aesculine screen. The practical details are too long for reproduction here, but they will, doubtless, soon be generally available. If plates so treated prove as advantageous as the author has found them, there seems to be no reason why autochrome, and perhaps other colour plates, should not be commercially supplied with suitable compensating screens. A reduction of the time of exposures to one-eighth would be an enormous advantage in almost all the work for which such plates are employed.

THE RESOLVING POWER OF PLATES.—In February last, I referred to a paper by Dr. Mees on this subject. Since then Dr. W. Scheffer has published (*Brit. Jnl. Phot.*, 1910, 453 and 576) the results of his experiments having the same object in view, but carried out in a rather different manner. Some of his conclusions seem so obviously true that many have probably taken them for granted and based their procedure upon them. But this in no way detracts from the value of an experimental proof of their truth. Dr. Scheffer employed as the object cross-lined screens, the opaque lines and the transparent spaces being of equal width, and also similarly ruled screens but with parallel lines only. A small image of this produced by a lens that was proved to have a greater resolving power than necessary, was photographed in a specially made camera, with due precautions as to focussing, and photo-micrographs were made of the resulting images. The communications are copiously illustrated with reproductions.

Each grain of silver salt upon which the light image falls, becomes luminous, and shines upon the adjacent particles. Dr. Scheffer finds that for the same quantity of silver salt per unit area, the resolving power is greater the more transparent, that is, the less turbid the film is. He finds that when the finest resolution is produced the image lies only in the surface of the film, so that the resolution of specially fine structure may be obliterated by the use of Farmer's reducer, dissolving it away, without much affecting the total density of the image. The lateral spreading of the light, which tends to obliterate detail or reduce resolving power, depends upon (1) the light scattering power of the film; (2) the transparency of the film; (3) the duration of the exposure; (4) the development. The best exposure is the shortest that will give a good image on development, but ten times this exposure deteriorates the resolution to a very small extent. One hundred times the exposure obliterates the fine detail. The best amount of development is that which would generally be considered as normal. If the gelatine is coloured of a tint that absorbs the active light, as yellow for ordinary plates, the lateral spreading is reduced and resolving power increased, and this is a matter that demands special attention in colour-sensitised plates, in which the film itself often remains coloured.

RADIO-ACTIVITY OF POTASSIUM COMPOUNDS.—The radio-activity of potassium compounds appears to be well established, and the failures to detect it due to the use of insufficiently sensitive methods. J. Elster and H. Geitel (*Physikal Zeitsch*, 1910, 275), have tried various methods to concentrate the activity of potassium salts, acting on the assumption that the activity might be due to some other substance than potassium (an impurity) present in all the tested sources of this metal. But they have failed every time to get evidence of such concentration or separation, and they therefore conclude that the activity is probably inherent in the potassium. The activity of potassium and other common metals is exceedingly slight, and doubtless would never have been observed at all if direct experiment had not been led up to by the properties of the notably radio-active substances.

PHYSICS.

By W. D. EGGAR, M.A.

THE PRESSURE OF LIGHT.—Suppose that a stream of minute corpuscles is flowing steadily against a surface which absorbs them and destroys their momentum. The momentum destroyed per second by unit area of the surface is a measure of the pressure of the stream. Now if m is the mass of the corpuscles in one cubic centimetre of the stream, and v the velocity of the stream at right angles to the surface, it is obvious that the mass impinging on one square centimetre in one second is mv, and, therefore, the momentum destroyed per second by one square centimetre is $mv \times v = mv^2$. In other words, the pressure of light, according to the corpuscular theory, should be equal to twice the kinetic energy of translation per unit volume of the beam of light. Many attempts were made during the eighteenth century to detect the pressure of light on a delicately-suspended disc; the results were quite inconclusive. Convection currents in the surrounding air, or, when a "vacuum" was employed, the radiometer effects, subsequently discovered by Sir William Crookes, interfered with the early experiments, as with the later ones of Lebeden, Nichols and Poynting. But the earlier experimenters, with no "principle of conservation" for measuring the energy of their beam of light, were unaware that the pressure for which they were looking was far too minute for detection by the apparatus which they employed. The corpuscular theory was abandoned and replaced by the wave-theory; but one of the reasons sometimes given for its abandonment, the non-existence of pressure, has in its turn to be abandoned. According to Clerk Maxwell's electro-magnetic theory, waves of light consist of electric and magnetic tubes of force at right angles to the direction of the beam of light, and these tubes should press on any surface on which they impinge with a pressure equal to the energy in unit volume of the beam of light. In 1899 Professor Lebeden detected and measured the pressure by letting the concentrated rays of an electric lamp fall on a blackened platinum disc suspended in a vacuum sufficiently high to get rid of convection and considerably to reduce the radiometer action; and, by using discs of different thickness with radiometer action proportioned to the thickness, he was able to calculate the pressure on a disc infinitely thin. His experiments, as well as those of Nichols and Hull, confirm Maxwell's theory. In the Bakerian lecture delivered at the Royal Society, last March, on "The Recoil from Light," Professor Poynting and Dr. Barlow describe their method of measuring the pressure of the beam of light against its source. If the beam of light carries momentum with it, as is now proved experimentally, it does not appear possible, whatever theory of light we adopt, to avoid the conclusion that this momentum was derived from the source of light. The experiments described seem to prove this conclusion, and the authors of the lecture proceed to consider the size of particles on which the pressure of light would balance gravitation. As the mass of a sphere varies as the cube of its radius, and the pressure of sunlight on its surface as the square of the radius, it is plain that spheres of radius smaller than the critical value (16×10^{-6} cm.) would be pushed away. This helps to explain the phenomenon of the tail of a comet. Again, in considering bodies for which gravitation is greater than the light pressure, a small sphere, absorbing the sun's heat and radiating its own, and at the same time moving round the sun, will experience a retarding force, due to the crowding up of the waves in front and opening out of those behind. Its energy will be gradually abstracted, and it tends to fall into the sun. A sphere $\frac{1}{10^4 \sqrt{10}}$ cm. in diameter at the distance of the earth from the sun would fall into the sun in 45,000 years.

ZOOLOGY.

By PROFESSOR J. ARTHUR THOMSON, M.A.

THE ZOÖLOGICAL CONGRESS.—The Triennial Meeting of the International Zoölogical Congress was held this year (August 15–20th) in Graz, a Mecca that rewards pilgrims not only in itself, "the pearl of the Steiermark," but in the journey towards it from any direction. A well-organised series of meetings, a large attendance of zoölogists from all parts of the Palaearctic region, an embarrassment of riches in the way of papers, a wholesome as well as beautiful town, a friendly and hospitable people, and plenty of sunshine, everything contributed to making the eighth Congress a success. In days of specialisms these meetings are more than luxuries; they enable one to keep within hearing at least of what is being

done in the many departments of zoölogy, and they give one a chance of meeting some of the few who know intimately about the corner in which one's own labours lie.

The first paper in the general section was by Prof. Yves Delage of the Sorbonne who discussed "Experimental Parthenogenesis"—one of the most interesting results of modern biology. Delage and Loeb and others, have shown that the unfertilised eggs of sea-urchins, starfishes, and some other types can be made to develop parthenogenetically under the influence of *a great variety* of artificial stimuli. The difficulty is to discover what is the factor common to all the different conditions which take the place of the spermatozoon in this, at least, that they set egg-cleavage agoing—pulling the trigger of development

Very striking also was what followed—a cinematographic exhibition of the development of the egg of the sea-urchin by M. F. Vlès and Mlle. Chevroton. With remarkable success the films showed the dividing of the egg into two, into four, into eight, and into a rotating ball of cells. The blastula became a gastrula before the eyes of the audience, and the gastrula grew into a pluteus-larva. It was a remarkable achievement to be able to show—with fine stereoscopic clearness—the whole process; and it was a great pleasure to see (as one may with a microscope) the quaint plutei rolling about in the water and growing from second to second.

Dr. Kammerer submitted to the Congress the results of a very interesting series of experiments on the transmission of acquired characters—a question to which he has already made an important experimental contribution concerning Salamanders. Briefly stated, his recent results are as follows. Certain Amphibians, Lizards, Gastropods, and Myriopods take on, wholly or partially, the colour of the substratum on which they live, *e.g.*, yellow earth or black earth. In some cases the progeny showed from the first a colour adaptation similar to that which the parents had acquired, or showed it more strongly than their parents did. Sometimes it was only shewn when the progeny were kept in the conditions which affected the parents. It is a good rule in such cases, however, to wait for the *third* generation, especially in instances where all that one can say is that the progeny show in the same conditions *more* than the parents did. They have been longer under the conditions in question, and they naturally acquire more.

Dr. O. Riddle reported on some experiments on pigeons and fowls as regards the formation of dark pigment (melanin). He regarded them as refuting the current Mendelian hypothesis of colour-development. What he found was that both the kind and the quantity of melanin pigment produced in the plumage of a bird depend on nutritive and other local and temporary conditions, and not on specific colour-determiners or "factors" in the germ-cell. Here again we have the familiar difficulty of balancing the claims of "nature" and "nurture." What we must hold firm by is the true platitude that what is expressed in development is the result of their coöperation.

One of the peculiar features of a number of recent experiments on nurtural influence is that there sometimes seems so little connection between the stimulus and its result. Thus Ch. R. Stockard points out that fish-embryos treated with weak alcohol or various anaesthetics tend to develop into one-eyed monsters.

One of the naturalist's primal impressions, that never seems to lose its wonder, is that of the abundance of life. In journeying to Graz we passed through part of Switzerland—renewing old impressions—and had again to marvel at the wealth of life at high altitudes. Thus at the Col de Balme, between Chamounix and Martigny, at an elevation of seven thousand feet or so, there were not only abundant flowers and butterflies, but plenty of whistling marmots, and several different kinds of birds in some abundance. Conspicuous were the large white-bellied Alpine Swifts, perhaps the most rapid of birds, continually swirling about, all in silence, in the cold air.

CORRESPONDENCE.

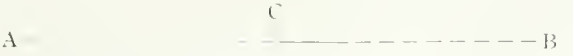
COMMENTS ON SOME FUNDAMENTAL IDEAS.

To the Editors of "KNOWLEDGE."

SIRS,—Without expecting too much from the analysis of questions under the above title, some irrefutable facts can, nevertheless, be isolated, while the whole may constitute some sort of recreation in reasoning.

The noteworthy distinction between what may be termed a statical (achieved) Infinite, *e.g.*, Space, and a kinematical (evolving) Infinite, *e.g.*, Time [connected with the idea of Motion], seems not uncommonly to be ignored. While, for example, the "antinomy" (or logical antithesis) joined to the idea of infinite *past* Time is usually noticed in philosophical treatises, the case of infinite *future* Time generally remains unconsidered. Nothwithstanding this, any difference in character between the two cases (which might present a pretext for this ignoration in the one instance) appears to be left out of account, and, indeed, the two cases treated as if belonging to exactly the same category or class.

Let AB represent a straight line prolonged in the direction from A indefinitely. Let AC represent the time achieved at any given epoch.



The point C (here indicated by a short transverse line) moves or slides towards B with uniform velocity. The line AC, which represents time of finite (definite) measurement, elapsed at any date, has two ends, in the particular sense that A and C persistently indicate two boundary points on the line, marking off finite Time accomplished.

So long as the motion of the point C takes place at a uniform rate, and the line AC does not jump out of itself *per saltum* (so to speak), it will lengthen uniformly and remain finite between the two points A and C.

It results that time in the future, actually accomplished, remains finite. Potential (infinite) time in the future, from the very nature of the case, can never become actual, for if an eternity could accomplish itself in the future, the gradual process of addition to time, which we now experience, would cease. Evidently, since the potential infinite remains always on the remote side of the point C, and the actual finite always on the near side of the said point [i.e., between C and A], this means that the potential infinite (as such) can never be transferred over, or realised.

There is no boundary assignable to the conceivability of gradual addition to the Finite, while the finite character is conserved in continuity. There is no barrier to the process of getting greater and greater without becoming more* than Finite by the process [a definite numerical value expressible in figures, by the choice of some convenient unit of length.]

If the straight line AC could undergo some occult variation in the act of lengthening, as some might tend to imply, such an irregularity would destroy or nullify the process of *gradually* lengthening,—would imply some catastrophic change in character. And a change in character is excluded while the lengthening process proceeds at a uniform rate,—which is the predicated characteristic of Time.

That which we might try to imagine, sometimes called 'the incommensurable' here, never arrives,—is never achieved. The concept here is a † kinematical concept (involving the

* For (it might be asked), how is a definite length AC to become *not* definite, by a predicated *uniform rate* of increase, when we can follow the process? The length surely must remain definite, however prolonged, or must continue to possess *some* definite value numerically expressible.

† In one respect at any rate, infinite *past* and infinite *future* time are not properly classable in the same category. In relation to the present moment, *past* time is stationary (statical), while *future* time (augmenting) is kinematical as a concept. Past time, when regarded as an infinitude actually achieved, is (in *this* phase) more comparable to space than to future time, *non*-achieved. *Space*, of course, if viewed as *non*-augmenting, becomes a statical concept, an infinitude accomplished or achieved.

idea of motion, augmentation), not a statical concept. This (last) affords an instance of 'the incommensurable' achieved, represented (say) by an infinite straight line, or by the infinitude of space (when regarded as something already present) which exemplifies a statical concept; since Space does not augment, while Time does.

Sir W. Hamilton and Mansel (quoted in Spencer's *First Principles*) object to any classification of Infinites among themselves; this implying, as they say, "plurality" of the Infinite. This is seen in the following passage, viz.:—

"How self-destructive is the assumption of two or more Infinites is manifest on remembering that such Infinites, by limiting each other, would become finite." (*First Principles*, p. 81).

But let us take the statical concept of space traversed by an imaginary straight line. We then observe that, harmonising with incommensurability, viewed towards the right the line is infinite in length, also, viewed towards the left the same can be asserted. But it is evident that these two "Infinites," by limiting each other, do not "become finite."

A prismatic column of space of (say imagined) square, or of any shaped (finite) transverse section, great or small, prolonged through space (entirely) would evidently, as to contents, enclose an infinite volume. And any number[1] of such universes (infinite in cubic content) could exist without mutual encroachments of territory, and even with lateral intervals of imagined unoccupied space, relatively narrow or wide between them.

In the instance of infinite *past* Time (in one sense a statical infinitude as remarked), that species of contradiction or 'antinomy,' peculiar to certain transcendental ideas, may be, at the outset, suspected. And here we have an infinitude [of time] *already* elapsed, whereas it is predicated of infinitude that it is never to elapse. And how is that to elapse which is not even to be begun?

A correlated 'antinomy' in the instance of infinite *future* Time, is by no means so easy to see; this essay having, so far, failed to discover any, and I am not aware if one has been definitely formulated.

The following consideration seems however to point to an antinomy in this case also, which might be suspected beforehand, from correlation with infinite *past* Time.

In regard to the coming future, some quantity (let us call it chronographic length) must be virtually transferred over, and located inside the point C, i.e., between C and A. If no *more* than a "*finite*" quantity or length be transferred inside the point C, the procedure (i.e., the lapse of time) will come to an end. Accordingly the length AC (notwithstanding the first argument) cannot "remain" finite, and must consequentially become *more* than finite.

But since we already recognised the inconceivability of setting a limit to the conservation of a Finite [AC] by a process of gradual (i.e., non-catastrophic) addition effected; therefore by such uniform rate of addition, the (elapsed) length AC must surely remain finite, however great it become. But we saw that by an opposing argument, apparently rivalling the first in its validity, the length AC must become *more* than finite. Hence antinomy discovered accordingly.

Some, however, might consider it a chief condition to cite the fact (say for illustration merely) that in order to parallel infinite future Time, an infinite supply of material for our ideal chronographic registering-strip would be required. But there is a noteworthy distinction between an inconceivability and an antinomy (or an antithesis in logic). For while the first constitutes something negative and indefinite, the second constitutes something positive and definite.

Still, it may be answered—The presentation of antinomies is not unusual by Infinitudes; but the fact may not necessarily invalidate their existence. A demonstration, one way or the other, may be difficult to furnish, and, whatever may be the true view here, one is at liberty to search and to point out what the antinomies are, indeed this procedure constitutes a needful and proper accompaniment to progress.

Yours faithfully,

S. TOLUER PRESTON, Ph.D.

KLEIN FLOTTBEK, BEI ALTONA, GERMANY.

April–June, 1910.

CONVENIENT SEATS FOR SMALL TELESCOPES.

To the Editors of "KNOWLEDGE."

SIRS,—The delightful little picture sent by Mr. Thomas; which was reproduced in the April number of "KNOWLEDGE," and his praise of an apple-case as a convenient seat, provokes me to utter a word of advice to the owners of small telescopes.

There are two accessories to the instrument, the absence of which will render it practically useless. This is a tall stand and a convenient seat. Mr. Thomas's stand looks as if it were just perfect for that small glass, but I demur to his "seat." I feel quite sure that the right way to use a small telescope is *to sit down first* and then to draw the legs of the stand towards you and around you till the eye-piece comes to the eye; the eye should not be put up to the eye-piece but the instrument brought to the eye. A deck chair answers well, and the final adjustment is to push a big handkerchief or some such cushion in behind the head till the eye or eye-piece are properly related. If the observer is in a strained position he is terribly handicapped. The attitude of that young observer is as hopelessly unpractical as it is perfectly charming.

Yours faithfully,

J. A. HARDCASTLE.

THE MOON AND THE WEATHER.

To the Editors of "KNOWLEDGE."

SIRS,—Having read Mr. Strickland's article in July number may I be allowed to follow in his footsteps?

Here, one hundred and fifty miles from the mouth of the Fraser River, and on its left bank, with us the wind decides the weather, and often the tides control the winds, and, as is accepted, the moon governs the tides.

The tides are not felt further up the Fraser than to the mouth of the Harrison River, yet the inrush of warm water causes, as an effect, a warming up of the atmosphere farther east.

Now I have in my office a record of the weather for 1905, 6, 7, and 9; 8 I missed. And in my memory records of many other years for the time of the full moons between December 21st and January 3rd, and the tides with this full moon are invariably extremely high all along the North Pacific coast, so that before Vancouver became a city, and man was somewhat primitive, the boat houses, boats and wharves suffered considerable damage. Now these tides cause a warming-up of the atmosphere and consequent displacement of cold, which is felt far inland. Whether the effect is the same East of the Rocky Mountains I cannot say, not having given any attention to the weather there. So noticeable is the effect along the banks of the Fraser, that what is known as a ground thaw is seen, that is, the ground being covered with ice, the ice becomes loose from the ground beneath it becoming soft from the warm current of air.

The weather means much to us, as we farm a large acreage and have to feed stock in winter, and we have to keep an eye on it, and to assist in a forecast we look overhead to observe the wind currents, which in this clear, bright part of the world can be seen pushing and fighting, cornering and dodging, and thrusting for the mastery—a battle of the clouds—and then we go to the almanac to see how the moon is, and work accordingly, taking things easy or rushing to get the hay or grain in, cattle housed, and so on.

GEORGE DITCHAM.

ST. GEORGE'S INDUSTRIAL SCHOOL FOR INDIAN BOYS, LYTTON, B.C.

[1] The (finite) breadth of transverse section of any *one* of such space-prisms of imagined infinite length, each capable of comprising a cosmos, might even be supposed comparable (say) to the estimated breadth of our (visible) stellar universe, or, of course, greater, if we like. Speculative thought, maintained within the range of the possible, may occasionally conduce to raise fresh questions

JOHANN GOTTFRIED GALLE.

By F. A. BELLAMY, M.A., F.R.A.S.

(*Continued from Page 315.*)

Besides the separate publications mentioned, Galle contributed papers to various Societies and periodicals: the only publication distributed in the name of Breslau Observatory, and edited by Galle, was a quarto volume of one hundred and sixty-six pages in the year 1879: its title is *Mitteilungen der Königlichen Universitäts-Sternwarte zu Breslau über die hier bisher gewonnenen Resultate für die geographischen und klimatologischen Ortsverhältnisse nebst einer Zusammen-stellung verschiedener, die Sternwarte betreffender geschichtlicher Nachrichten und darauf bezüglicher Zahlenangaben und Tabellen.*

Another important investigation in which Galle was the greatest contributor, was upon the disappearance of Biela's comet, which had been observed, in December, 1845, to divide into two, and has not been seen since. Galle, D'Arrest (who was Galle's junior colleague at Berlin in 1847) and the veteran Dr. E. Weiss, who is still living, working along the lines of Schiaparelli's investigations, who was doubtless incited by H. A. Newton's highly successful accomplishment in dealing with the Leonids, were able, independently, to establish beyond doubt, that the orbit of Biela's comet and the radiant point of the unusual displays of meteors observed in 1798, 1838, 1847 and other dates were coincident: this stream or swarm of meteors was travelling in the same path as that which Biela's comet occupied until its final disappearance as a comet: their papers are in the *Ast. Nach.* for the year 1867. Though their evidence was strong, some wanted proof that those meteors, now called Andromedes*, which periodically appear near γ Andromedae, were portions of the disintegrated comet known as Biela's. As the periodic orbit of the comet was 6·6 years at a mean distance of 3·5 from the sun, one had not to wait long. It was calculated by Weiss and Galle that in 1872 the comet would be nearest the sun, and the orbit would nearly intersect that of the earth in November, when the earth would be in a similar position in space: they predicted that, instead of the lost comet appearing, its place would be taken by a meteoric shower or display: Galle predicted the day to be November 27th: he was not twenty-four hours wrong, for at sunset on November 28th the Andromedes storm broke forth and meteors appeared at the rate of some hundreds a minute, as did also a number of immense fireballs. At the return of the swarm in 1885 (November) I was a fortunate observer, though I could only spare odd minutes from transit-circle work. In that year the display was well observed, and considered to surpass in magnificence and number any appearance before or since: in fact the recent displays have dwindled and been so poor that there is a probability that the earth now only encounters the outlying members of the stream.

If none of the facts stated were sufficient to carry Galle's name to posterity as an important discoverer of astronomical phenomena, and to keep his name green and highly-esteemed among astronomers and in all observatories, there is still one other discovery of historical and epoch-making importance to the credit of Galle not yet referred to: it was he who first knowingly observed the planet Neptune.

To deal adequately with the complete account of the discovery of Neptune would occupy many pages of this magazine, and far exceed the length intended for this article. Nevertheless, any account of Galle would lack completeness and justice to his name which omitted some detailed reference thereto: it is hoped that endeavours to keep the account to brief limits will neither omit important facts nor fail to give due praise to others who were concerned in the discovery of the planet.

The discovery of Neptune is among the most interesting, most brilliant, and most important achievements in Astronomy of the nineteenth century. About the year 1843 J. C. Adams commenced his investigations upon the disturbances which affected the movement of Uranus: after two years work he informed Prof. Challis of the result of his calculations, which was to the effect that he gave the approximate elements and the position in which the planet must be at a particular time. In 1845 (October), Adams also sent similar information to the Astronomer Royal, but owing to the proverbial red tape and super-scrupulousness, and to the natural disinclination to help on others, for, to use his own words, he has stated "I have not directly contributed either to the theoretical or to the observing parts of the discovery," the matter was shelved until Le Verrier, who had, also in 1845, been independently investigating the movements of Uranus and the probable existence of another and unknown disturbing planet, read his second paper before the French Academy in 1846 (June): his third paper was read on August 31st and predicted the position in which the body would be found. Le Verrier's second memoir was in Airy's hands in 1846, June 24th, and he was evidently surprised to find that the predicted place was only one degree from that predicted by Adams nearly a year before. Some attempts were made in July and August to recover the nine months lost, from the search in England, but

* Or Andromedids as more generally used.

too late again; so neither the priority of the theoretical nor the visual discovery could be placed to the credit of this country—though England participated in it in some degree.

In the *Ast. Nach.*, No. 2134 (for 1877) Galle has given a historical account of the visual discovery. He said that in 1845 he had sent Le Verrier a copy of his dissertation, just printed for his doctor's degree; to this Le Verrier did not reply until he wrote in 1846, September 18th, and suggested that Galle should use the large equatorial and search for the unknown body, and that he would probably detect it by its disc. This letter reached Galle on September 23rd, and he determined to look for it. It was fortunate that the Berlin Observatory possessed a copy of the newly-published map—being Bremiker's map for R.A. XXI^h. Its use was in aiding the almost immediate discovery of the planet, and, according to some accounts, it was suggested by D'Arrest. Galle used the 9½-in. telescope, whilst D'Arrest checked off the objects as Galle observed them. On the evening of September 23rd Galle very soon observed an object, equal to an eighth magnitude star, which was not on the Berlin Academy's charts; he, and Encke afterwards, measured its distance from the surrounding stars and made its diameter to be 2″·7, which agreed very closely with that calculated by Le Verrier. The longitude heliocentric from observation on September 23rd was 326° 57′, and that calculated from Le Verrier's elements was 326° 0′, being less than one degree in error—a wonderful accomplishment. Observations by Galle on September 24th and 25th established beyond any doubt the planetary character of the body.

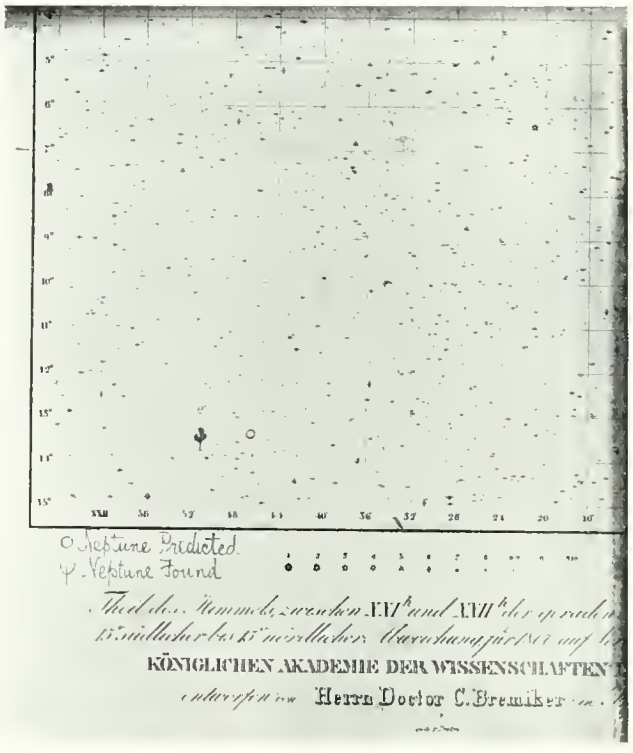


FIGURE 1.
Portion of the Berlin Chart, by means of which Neptune was discovered by Galle.

In the same year, 1846, Challis had been stirred, by the publication of Le Verrier's memoir in June, to use the telescope to search for the object, and the scrutiny was actually commenced on July 29th, though, had Adams' calculations been treated more respectfully, there is no adequate reason why the search should not have commenced in 1845 (September), with the most probable result of Le Verrier and Galle being forestalled both in the theoretical and visual discovery. Challis's method was to note down the approximate position of all stars in the selected region to the eleventh magnitude, and then to repeat the process at intervals and compare the results. In this way he hoped to detect any strange object. On July 29th he scrutinized the region calculated by Adams, also on July 30th, August 4th, August 12th and later days, but he delayed the comparison of his observations. Oh! these fatal delays. Some time later, when the comparison was made, he found that he had actually observed the planet on August 4th—only five days after starting the search—and again on August 12th, of the eighth magnitude. He said that he would have undoubtedly detected the planet sooner and in a far less laborious manner had he had any map with which to compare his observations. Again, on September 29th, he observed the planet for the third time, and that time more consciously, as he noted one among the 300 stars observed "seems to have a disk," and that was before he had received any intimation from the Continent that the planet had actually been discovered by Galle on September 23rd. If priority of publication is to be the ruling spirit in scientific discoveries then England cannot claim, unfortunately, mainly through the action of two men, Airy and Challis, the honour of either discovery just discussed. Galle visually discovered Neptune.

The planet had, however, been observed as a *star* by Lalande so long before as 1795, May 8th and 10th, and by Lamont in 1845, October 25th, and 1846, September 7th and 11th. Correspondence and notes concerning the part that D'Arrest took in the discovery may be found in the volumes of the *Ast. Nach.* and *Copernicus*.

The planet was not at first called NEPTUNE, but LE VERRIER, after its discoverer. Galle, in a letter to Le Verrier, suggested that the name should be JANUS. Le Verrier replied that "Le Bureau des Longitudes s'est prononcé ici pour Neptune, le signe un trident," and continued "Le nom de Janus indiqueroit que cette planète est la dernière du système solaire, ce qu'il n'y a aucune raison de croire."

As mentioned at the beginning of this note (p. 314), J. G. Galle was born on June 9th, 1812, at Pabsthaus by Gräfenhainichen, between Wittenberg and Bitterfeld, where his father carried on a tar-kiln business. Galle was the eldest of seven children, and

received his early education at the village school, but he was also taught, with the son of the pastor of that place, the older languages; later on he entered the gymnasium in Wittenberg, where we learn that its director knew, with joyous pride, that from 1825–1830 Galle was considered to be, and proved himself to be, its most eminent and distinguished scholar, particularly in Latin—in which language he wrote his dissertation in 1845. From the age of eighteen to twenty-one (1830–1833) he assiduously studied mathematics and astronomy in Berlin, receiving instruction from Prof. J. F. Encke; in 1833 he assisted as examining headmaster, and directly after he held a position for half a year in Güben, and then in the Friedrich-Werderschen Gymnasium, in Berlin.

In 1835 he gave up these teaching positions and was appointed the Observer, under Encke, at the new Berlin Observatory, and continued his work and investigations there until 1851 in a most careful, assiduous, and conscientious manner.

In 1851 he went to Breslau University and Observatory, where his work for forty-five-and-a-half years was both as a teacher and investigator. Soon after his arrival there he was convinced of the great necessity for the formation of a purely mathematical and astronomical curriculum, hitherto deficient.

His lectures were made to embrace many branches of astronomy; and by his gifted, luminous, and extremely clear manner of explaining difficult questions, he attracted students, claimed their attention at his lectures, and thereby introduced many students to astronomy. The number of his audience increased and attained, about 1890, then for astronomy only, to the unusual height of over eighty. His scientific work kept him occupied and interested in all current questions. The climatological tables prepared by him from forty years' observations at Breslau are still in use. Some of his non-astronomical researches have appeared in the proceedings of the *Schlesischen Gesellschaft für vaterlandische Kultur*. He founded and carried on the geographical section of this society, and, for his esteemed work, was made an honorary member.

In the publication of the Breslau Observatory, in 1879, he gave a faithful historical account of the origin of the astronomical lectures, and of the observatory in Breslau, so far as concerned building changes, and finally prepared a catalogue of all existing instruments and their origin, the geographical, meteorological, and magnetical constants for Breslau. And in 1897, when his lectures and connection with Breslau came to an end, he had formed an inventory of the Observatory and contents of the library, and surrendered this with his resignation of office: these testify to his methodical way of doing things thoroughly and leaving no work undone or in disorder for others.

Galle married the sister of Herz, Professor of Classical Philology at Breslau, and of this marriage two sons were born. The elder, Andreas, became, to Galle's delight, like him, an astronomer, at first at the Breslau Observatory and later at the Royal Geodetic Institute at Potsdam, where he still holds a respected and influential position as Observer, as well as Professor of Geodesy to the Technical High School in Charlottenburg (Berlin); the younger son, George, became a physician in considerable request in Erdmannsdorf, and later, in Hirschberg.

After the death of his wife Galle's niece, Miss Marie Lindemann, undertook the management of his household arrangements, and devotedly attended to him until his death. When Galle resigned his positions at Breslau he changed his residence and, at the request of his son, Dr. Andreas Galle, went to live near him at Potsdam for the last thirteen years of his life.

On the occasion of his ninetieth birthday on June 9th, 1902, a deputation of the International Astronomical Society, of the Astrophysical Institute at Potsdam, of the Berlin and Breslau Observatories, and of other scientific bodies, came to congratulate him upon the rare celebration. This old man, in vigorous health, stood to listen to all the addresses, and continued standing while replying to each one singly.

All letters which he received upon his ninetieth birthday, on later birthdays, and at the New Years, he himself answered in detail; in his own particularly firm, clear, and beautiful handwriting, little changed from that of his early manhood. Not until his last, the ninety-eighth, birthday on June 9th, did he allow a son to undertake the replies. He enjoyed tolerably good health on June 9th, and was only ill for about ten days before his death on Sunday night, July 10th. Galle, besides being a Professor and having a Doctor's degree, was a Privy Councillor, Emeritus Director of the Breslau Observatory, a Foreign Member, or Associate, of The Royal Astronomical Society (London) for the long period of 62 years (since 1848), being the "father" of that Society, and a Corresponding Member of the Royal Bavarian Academy of Sciences.

At first, when Neptune was discovered, it was generally considered that the discoverers were Le Verrier and Galle. But Galle himself, in his retiring and most modest manner, always stedfastly declined to be honoured as the discoverer, and designated Le Verrier as the sole discoverer of Neptune. Notwithstanding his views upon the matter, the Cross of the Legion of Honour was sent to him from Paris.

Let us conclude this account with the words of Dr. Franz. Fidelity and carefulness, combined with his geniality, are the attributes which distinguished Galle: and if Germany has become in the last fifty years so conspicuously prominent among European nations, we are indebted to its union by Bismarck and to such men as Galle, who in quiet work and German thoroughness have elevated and stamped truth on its shield.

The writer is greatly indebted to Galle's son, Dr. Andreas Galle, for much personal information and for the portrait from which the frontispiece to the present number of "KNOWLEDGE" has been taken.

NOTES ON BRITISH FORAMINIFERA.

III.—POLYSTOMELLA CRISPA LINNÉ, SP.

By EDWARD HERON-ALLEN, F.L.S., F.R.M.S., AND ARTHUR EARLAND.

NEXT to *Massilina secans* in the order of frequency of occurrence in most English shore-gatherings comes the beautiful species, *Polystomella crispa*. If we may judge by the exclamatory opinions expressed by casual observers at microscopical *soirées*, this is by far the most exquisite of all the Foraminifera, and its extreme commonness, combined with its highly complex shell formation, has attracted to it a lion's share of scientific study and observation.

In domesticity, if we may so call the life of these Rhizopods in our observation tanks, it is nearly as amenable to its quasi-artificial conditions as *Massilina secans*, and as soon as the latter emerge from the mud they will be seen to be accompanied by the more robust *Polystomellae* on their journeys up the glass or among the growing algae. In this state they present a magnificent appearance: the peripheral edges, and, as a rule, the terminal chamber, are seen to be quite transparent, and the sarcode body, of a deep reddish-brown tint, is clearly visible inside the shell. In this species the difference between the deeply coloured sarcode and the transparent layer of protoplasm from which the pseudopodia are produced, may be very distinctly observed. The pseudopodia are sparingly extruded from all the pores of the shell, producing anastomosing filaments all round it, whilst at the terminal aperture a principal group of long pseudopodia are extended in a fan-like arrangement, sometimes to a length several times the diameter of the shell. These terminal pseudopodia are the first to be extruded when the animal emerges from the mud, and appear to have a specialised function of adhesion and locomotion. When no peripheral or surface pseudopodia are visible the animal may be seen under a low power of the microscope to be squirming about on its aperture, apparently seeking for a suitable spot on which to anchor itself.

FIGURE 1.

Polystomella crispa (Linné, sp. Specimens showing the shell in all stages of growth × 30. The specimens (2) at bottom show the shell in edge view with aperture.

The shell of *Polystomella crispa*, though always true to a certain type of structure, varies enormously in appearance according to the locality and its surroundings, which no doubt explains the plethora of names from which it has suffered. But our British specimens do not present any great range of variation, although, in any large gathering, specimens may be found which bridge the interval between it and the closely allied species *Polystomella macella*. Typically, a British specimen of *P. crispa* is a biconvex and bilaterally symmetrical nautiloid shell. The convex surfaces may be either flattened and compressed like the familiar "tabloid," or the convexity may be pronounced with the central umbo standing prominently out from the surface of the shell. The chambers are arranged in an involute or embracing spiral of several whorls. Each individual chamber is V-shaped so that it rides over and embraces its predecessors in the inner whorls. This overlapping of the chambers does not quite extend to the centre of the shell, and the umbilical depression thus left is filled up by a deposit of solid shell substance, forming an umbo, or boss, which is perforated by a system of canals, which also link up the chambers and through which the protoplasm circulates. As a result of the overlapping of the chambers and the secretion of the central umbo, only the last convolution of chambers is visible externally. In appearance the shell varies from a glassy transparence in living specimens, through milky translucency to a dead white in shells where the animal has been dead a long time.

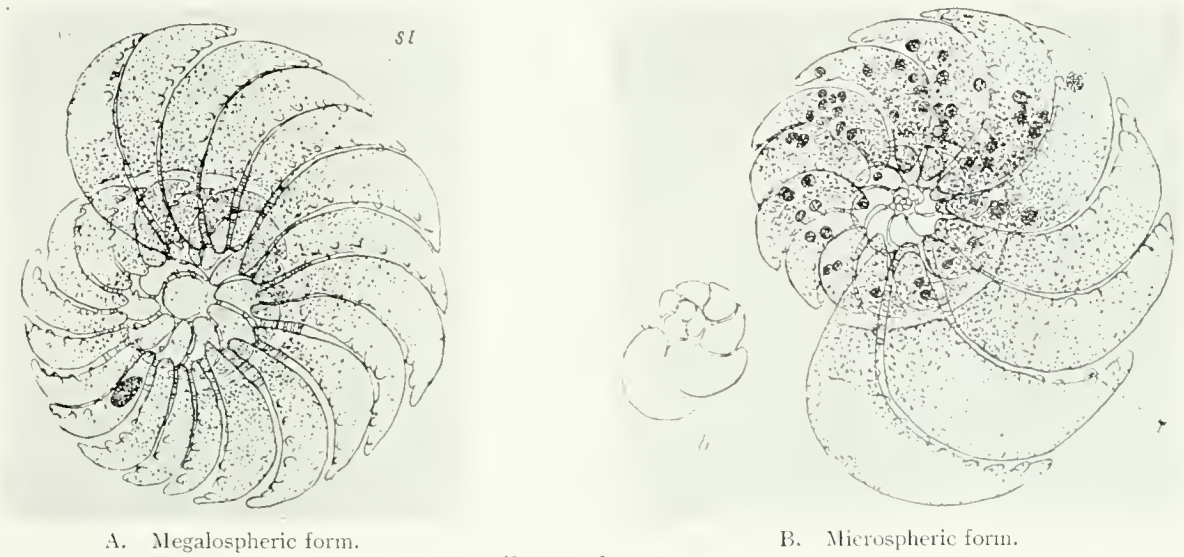
Massilina secans with which we dealt last month, belonged to the porcellanous or imperforate group of Foraminifera, which have but a single aperture and

no perforations in the shell wall. *Polystomella crispa*, on the other hand, is a representative of the hyaline or perforate group, which, in addition to one or more special apertures, have the shell wall perforated with innumerable fine pores through which the pseudopodia can be extruded. The special aperture in the case of *P. crispa* consists of a line of V-shaped pores situated on the face of the last chamber, close to the wall of the preceding convolution. These pores, which are the true "foramina," in the original and strict sense of the word, form the principal channel for the passage of the protoplasm from the animal, but in addition there are countless numbers of extremely minute tubuli through the shell substance, which are so fine as to be visible only by transmitted light and under a fairly high power of the microscope.

The external surface of the shell is covered with a series of raised bands, radiating from the central umbo in a series of graceful curves. These are the septal ridges, and mark the divisions between the separate chambers of the shell. Projecting backwards between the septal ridges, and at right angles to them, are a series of smaller ridges marking the surface of the wall of each chamber. These are the coverings of the so-called "retral processes," which are pockets in the shell substance into which prolongations of the protoplasmic body extend. The "retral processes" are a characteristic feature of the genus *Polystomella*.

The peripheral edge of the shell may be more or less rounded, but it is generally produced into a sharp keel, and often marked by a deposit of clear shell substance. In very young shells the points of intersection of the septal ridges and the keel are marked by the growth of strong spines, which are prolongations of the septal ridges. These spines decrease in size and number as the shell increases in size, but they can often be traced in specimens of considerable age. A full-grown specimen averages one-fifteenth of an inch in diameter.

Owing to the abundance of *Polystomella crispa* and its readiness to adapt itself to an artificial existence, it has formed a favourite study for the life-history of the group with many specialists. The result is that we know its life-history with greater completeness than that of any other Foraminifer, principally owing to the exhaustive researches of Mr. J. J. Lister, F.R.S. Much of what we now know as to the curious "*dimorphism*," long recognised in the Foraminfera, is based on his discoveries in connection with *Polystomella crispa*.



A. Megalospheric form. B. Microspheric form.

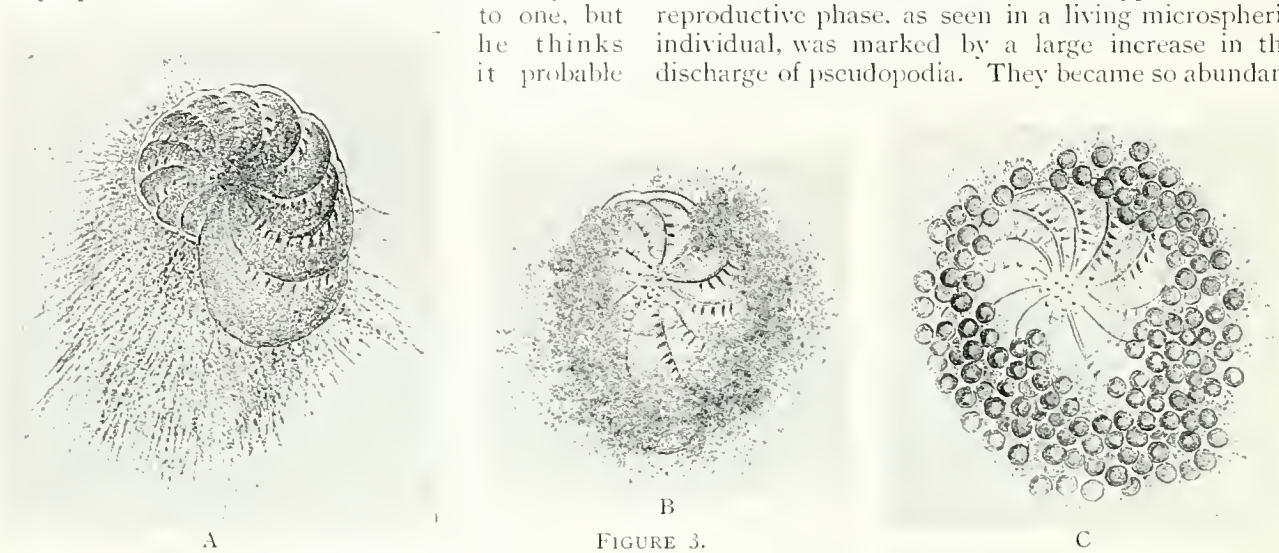
FIGURE 2.

Polystomella crispa decalsified to show the sarcode. *b*, the central chambers of the microspheric form more highly magnified. The canal system is omitted. Nuclei deeply stained. *r* = retral processes. *st.* = stolon or communication between chambers.*

First, let us give a brief account of *dimorphism*. It was very early recognised, from the study of Nummulites, that in a series of specimens from one gathering, the majority were of one fairly uniform size, while a small minority were of much larger dimensions. These two types, although occurring in the same deposit, were generally regarded as separate species, and many theories were formed to account for their constant and mysterious association. Closer examination of the specimens showed that the small specimens commenced their existence with a large primordial or initial chamber, and that this was followed by relatively few convolutions of chambers. The large specimens on the other hand were found to commence life with a small primordial chamber followed by many convolutions. The type with the large primordial chamber is called the *megalospheric* form or Form A, while those with the

*Figures 2, 3 and 4 are reproduced by kind permission of Mr. J. J. Lister, F.R.S., from his monograph on the Foraminifera in Lankester's Treatise on Zoology.

small primordial chamber are known as the *microspheric* form or Form B. The microspheric type is always much less abundant than the megalospheric. In *Polystomella crispa*, according to Lister, the proportions of the two forms are as thirty-four to one, but he thinks it probable that the proportions may vary with the season.



A B C

FIGURE 3.

Stages in the reproduction of the microspheric form of *Polystomella crispa*.

Lister's methods and results are to be found in the papers referred to in the Bibliography at the end of our note. We present a brief summary only.

Living specimens were killed and fixed by means of a warm saturated solution of corrosive sublimate and glacial acetic acid in the proportions of four to one. This had the effect of rapidly dissolving the calcareous shell while preserving the protoplasm body as a perfect internal cast of the shell. All the bodies, irrespective of size or stages of growth were found to be separable into two groups, megalospheric and microspheric. In the megalospheric specimens the "retral processes" characteristic of the genus were observed in the chamber next to the megalosphere, but in the microspheric specimens the "retral processes" were not to be seen in any of the early convolutions. On staining the protoplasmic bodies with picrocarmine it was found that the megalospheric and microspheric forms were also sharply contrasted in the number and character of their nuclei, the megalospheric form having but a single large nucleus while the microspheric form possessed many small nuclei distributed throughout the body. (Figure 2, A and B).

The relationship of the megalospheric form to the microspheric became apparent when the life-history of the animal was worked out. The approach of the reproductive phase, as seen in a living microspheric individual, was marked by a large increase in the discharge of pseudopodia. They became so abundant as to form a halo round the shell (Figure 3, A). This was at first clear protoplasm, but subsequently the brown granular matter passed out of the shell until all its protoplasm was outside, leaving the shell empty (Figure 3, B). After streaming movements in the extruded protoplasm, it separated into spherical masses of equal size, each surrounding a nucleus. Each spherical mass secreted a calcareous shell with a single aperture (Figure 3, C). The spherical masses, which were of identical size with the primordial chambers of megalospheric specimens, then began to separate and disperse (Figure 4), and shortly after each one formed a second chamber, which possessed the characteristic "retral processes," and was recognisable as a young megalospheric individual. The whole process, from the formation of the halo to the dispersal of the young brood, occupied about twelve hours.

FIGURE 4.

A later stage in the reproduction of the same specimen showing the formation and separation of the young megalospheres.

It was thus proved that a microspheric adult produced megalospheric children. Following up the history of these, Lister found that as they increased in size and number of chambers the nucleus also increased in size, and moved onward through the

successive chambers, becoming constricted in order to pass through the pores connecting the chambers. In full-grown specimens the nucleus was usually found midway between the earliest and the latest chamber. Later still the nucleus was observed to break up and disappear, and a great number of nucleoli made their appearance in the protoplasm. Each of these nucleoli attracted to itself, and became surrounded by a minute rounded portion of protoplasm. The nucleoli then divided by karyokinesis, and subsequently the whole contents of the shell poured out as active flagellate zoospores, and swam away.

Here the direct observation of the life-history ended, for Lister was unable to follow up the movements of the active zoöspores. But there is every reason to believe that after a free swimming existence the zoöspore settles down as a microsphere, and so commences the life cycle anew. Lister points out that the zoöspores are on an average less than one-third of the size of a microsphere, and he records an observation of Schaudinn, who found that when cover-glasses were suspended in an aquarium with living *Polystomellae*, so that they hung free from the sides and bottom, young microspheric specimens made their appearance on them in two days. He points out that *Polystomella* can only crawl over surfaces and cannot swim, and that the colonisation of the coverslips under such conditions implies the existence of an earlier free-swimming stage, such as the zoöspores supply. Moreover, he points out that the difference between the size of the zoöspores and the microspheres would be explained if a sexual conjugation of the zoöspores took place, such as has been observed in the case of other organisms producing motile spores.

Lister concludes from his observations that the megalospheric and microspheric stages represent an alternation of generations. While the megalospheric generation is produced asexually from a microspheric or, occasionally, a megalospheric parent, the microspheric generation arises sexually from the conjugation of two similar zoöspores, probably derived from different parents.

It is difficult to write with certainty as to the distribution of *Polystomella crispa*, owing to the fact that the records of this species have been confused with those of its ally *P. macella* Fichtel and Moll. sp. Brady states that it has been recorded from as far North as Disco Bay, Greenland, and as far South as Kerguelen. Probably, however, these extreme records refer to *P. macella* (which certainly has a wider range than *P. crispa*), although Brady, for some unexplained reason, fixes the Mediterranean as the Northern limit for that species. It would appear, however, from our own observations that *P. crispa* is to be found from the Equator to Fetlar in the North of Shetland at any rate. Goës records it from the Skagerack and the Swedish coast. South of the Equator it extends to the Southern Island of New Zealand, but apparently this is its southern limit as Chapman does not record it from "The Snares" (which are at the extreme south of the Islands), in his recent paper on Sub-Antarctic Foraminifera. In depth it ranges from shore pools to three hundred and fifty-five fathoms, according to Brady. We ourselves have found a single specimen in a dredging taken by the "Challenger," off the Azores, in one thousand fathoms, but this may be an exceptional individual, derived from a piece of floating alga.

Polystomella crispa was one of the first Foraminifera to attract the attention of those conchologists who studied what they considered to be minute Mollusca. It was figured and described by *Plancus* so far back as 1739, and again by Gualtieri in 1742. It was observed by the great Linnaeus to whom it owes its name. Needless to say it has suffered considerably from the species-mongers in the interval since Linné christened it, and a complete Bibliography of the species would extend to a considerable length. As, however, most of the synonyms refer only to feeble or intermediate varieties of the type, we confine our selection to the most important records. Those interested in the subject can extend the list to very much larger dimensions by referring to Mr. Sherborn's invaluable Index.

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REVIEWS.

GENERAL.

Science from an Easy Chair.—By Sir Ray Lankester, K.C.B., F.R.S. 5¼-in. × 7¾-in. 423 pages. 82 figures.

(Methuen & Company, Limited. Price 6 -.)

There is an ever increasing need for the popularisation of science, not as understood by many journalists (which to them means the exaggerating of a scientific statement, or the parts of it which suit their purpose, and the attenuating of its truth), but for those who really know their subjects to pick out those points which appeal to others who have no technical knowledge, and to clearly and lucidly describe them. In this way the interest of many in the world around them is greatly increased, and the influence spreads and grows stronger, so that their children, if not they themselves, may ultimately take up science seriously. In fact what we want is more scientific men like Sir Ray Lankester, who can write as well as work. The essays which go to form this book are articles which have been reprinted from *The Daily Telegraph*, and the author has taken a great deal of trouble to introduce some striking and interesting illustrations. Sir Ray Lankester claims that the coloured drawings of the adult silver eel and the immature yellow eel are the first that have been published. Others we may mention are the prehistoric engraving of a horse's head with a rope bridle, and of the jumping bean and the moth which comes from the grub within it. In the preface a hope is held out that other volumes will follow, and we feel sure that if they do Sir Ray Lankester may be congratulated quite as heartily as in the present instance.

Idols of Education.—Selected and Annotated by Charles Mills Gayley. 4¾-in. × 7-in. 181 pages.

(Hodder & Stoughton. Price 2 6 net.)

After an introduction which is an evident attempt at fine writing that is rather wasted upon us, probably owing to its being too reminiscent of Latin prose, the author proceeds to point out how, in the American Colleges at least, the time is mostly taken up with meetings, rehearsals, preparations, and rubbish, and he endeavours to set up other idols in their place. Professor Gayley strongly criticises the system of Rousseau and Froebel, and seems to think that the idol of play is undermining all discipline. He also finds a source of trouble in the fact that the greater percentage of teachers in America are women. Those interested in education should read the book, and those responsible for teaching in this country should enquire whether the strictures of Professor Gayley may not soon be applicable to England.

BOTANICAL.

Summer Flowers of the High Alps.—By Somerville Hastings. Illustrated by Reproductions from direct colour Photographs by the Author. 6¾-in. × 8⅝-in. 85 pages, 39 plates.

(J. M. Dent & Sons, Limited. Price 7 6 net.)

Mr. Hastings' book is most interesting, because the photographs from which the plates were made were taken on Lumière plates which give the colours in a transparency. The process, it will be remembered, depends upon the action of light after its passage through various coloured starch grains. The transparencies made by Mr. Somerville Hastings have been reproduced by the three-colour process, that is to say the plates have been printed from three half-tone blocks. It is no good deceiving oneself with the idea that this process is yet perfected, and those who know the flowers will miss some of their more delicate tints, while the colours of the backgrounds are unduly emphasised. As the writer had the privilege of seeing a number of the original transparencies some time ago, he cannot help but say that they are in many cases much better than the reproductions. The book will serve as an excellent introduction to the study of Swiss Flowers, and Mr. Hastings gives references to other books which the student may afterwards consult. In ihs introduction, Mr. Hastings briefly contrasts the flowers of the Alps with those of other countries, and he says that we are so used to the bluebells of our woods, the purple heather of our heaths, and the yellow gorse of our commons that we hardly realise how glorious they are. In Switzerland the gorse is very rare, the bluebells are not found at all, and the bell-heathers are absent, though the ling is found everywhere, but, like many others, Mr. Hastings finds the flowers of the mountains truly wonderful and his pictures, taken from the plants as they were growing, will go a long way to prove that they are.

How to Know the Trees.—By Henry Irving. 5¼-in. × 7¾-in. 179 pages. With illustrations from photographs by the Author.

(Cassell & Company, Ltd. Price 3 6 net.)

Trees very naturally lend themselves as subjects for nature study. Their varied shapes, their seasonal changes, their bare branches, or their twigs covered with foliage, are all attractive, and as if these details were not enough there are the manifold shapes of their leaves, their various barks, their evident or inconspicuous flowers, and if one likes to go further than mere outward observation there is their internal structure and the consideration of the part which they play in the world of Nature. Mr. Irving has done his share to advance the study of trees. His photographs have embellished the cases of many museums, and the pages of other men's books, and in the volume before us he adds descriptions in his own words of the trees that he pictorially represents.

It suffices us to say that all our common trees are dealt with, and these are shown in the illustrations as they appear in summer and winter in their entirety, while all the details to which we have made allusion are carefully figured with the help of the camera.

The book is well got up, and should encourage and help the nature student very considerably.

PERIODICAL.

Science Progress in the Twentieth Century. A Quarterly Journal of Scientific Work and Thought. Vol. V. No. 17, July, 1910. Editors.—H. E. Armstrong, Ph.D., LL.D., F.R.S., J. Bretland Farmer, M.A., D.Sc., F.R.S., and W. G. Freeman, B.Sc., A.R.C.S., F.L.S.

(John Murray. Price 5/- net.)

The first article in the current number of "*Science Progress*" consists of an account by Professor H. H. Turner of the great star map which, with the help of photography, is being made by astronomers working in different parts of the world. He points out that there is very little technicality about the process which is not familiar to the user of an ordinary Kodak. The astronomer uses a much longer camera, which he drives by clockwork so that it may follow the stars, and he takes pictures at night instead of in the daytime, using the light emitted by the stars instead of that reflected from the sun. Incidentally an account is given of the way in which photographs of stars came to be taken. The present scheme includes not only the pictorial representations on the plates, or prints made from them, but also the measurement of the plates, and the publication of the measures of original stars. The account of the work is to be continued.

Dr. Florence Buchanan describes a number of experiments made in connection with the pulse rate of animals. The object is to show the power of the heart in different species of warm-blooded animals of complying with the demands made upon it not on occasion only but for life. A very interesting apparatus is figured showing the way in which the pulse of a bird is taken. Other articles are those on New Theories of the Evolution of Stellar Systems, by F. W. Henkel, and on Agricultural Progress in the Tropics, by Dr. J. C. Willis.

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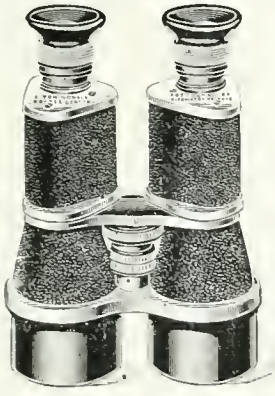

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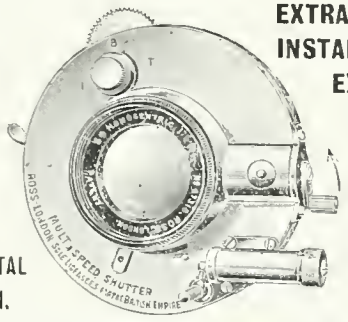

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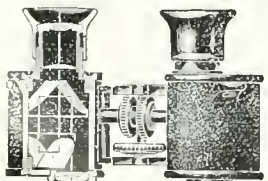

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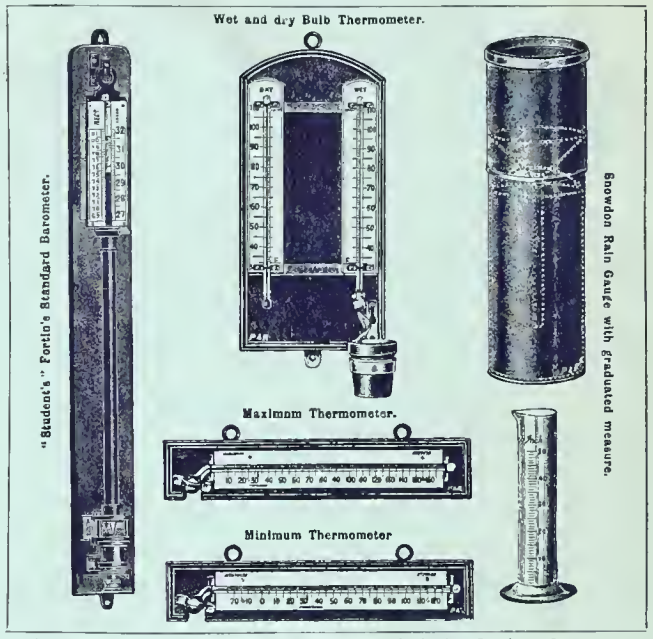

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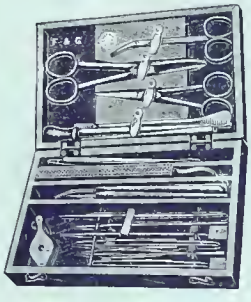

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